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Cost-benefit analysis and its application to crime prevention and criminal justice research

Kym Dossetor

AIC Reports
Technical and
Background Paper **42**

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Foreword

Cost-benefit analysis is one of a number of ways of measuring the impact and effectiveness of crime prevention programs and has been used by governments in Australia and elsewhere to determine the utility of a given program or intervention. In essence, cost-benefit analysis is an analytical tool that compares the total costs of an intervention or program against its total expected benefits; it assists in answering the question 'has the money been well spent?'.

Application of cost-benefit analysis within the criminal justice system and the crime prevention field is increasingly being embraced, although to date, most work has been undertaken in the United Kingdom and the United States. By comparison, relatively few cost-benefit analyses have been completed within Australia in these fields.

In this report, a description is provided of when and how such analyses of crime prevention programs have been used and a number of cost-benefit analyses are reviewed, using a tool developed to

assess the merit of cost-benefit analysis. It is noteworthy that a number of the programs that have shown a reduction in the risk of crime have not been developed by criminologists or law enforcement personnel, nor has crime prevention been the primary objective. Rather, a crime prevention effect has occurred as part of a suite of positive outcomes.

For policymakers, cost-benefit analysis can be an important tool that informs policy decisions around program continuation, expansion or cessation. For practitioners, knowledge that programs are achieving their intended goals can assist program managers in future program development and may help to justify program expenditure.

This report improves our understanding of the application of cost-benefit analysis and provides ways in which to make this important analytical tool more responsive and effective, which will help to ensure sustained investment in quality crime justice and crime prevention programs.

Adam Tomison
Director

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Acronyms

AIC	Australian Institute of Criminology
AWC	Anchorage Wellness Court
CBA	cost-benefit analysis
CBT	cognitive behavioural therapy
CCTV	closed circuit television
CEA	cost-effectiveness analysis
CJSC	crime and justice steering committee
CPC	Chicago Child–Parent Center
CRD	Centre for Reviews and Dissemination
DTO	Drug Treatment Order
DUI	driving under the influence
GIS	geographical information system
HIT	high intensity training
ISM	intensive supervision and monitoring
JCI	juvenile corrections institutions
MJTC	Mendota Juvenile Treatment Centre
NCPC	National Crime Prevention Centre
NFP	Nurse–Family Partnership
NMVTRC	National Motor Vehicle Theft Reduction Council
PNC	Police National Computer
RBI	Reducing Burglary Initiative
RIS	Regulatory Impact Statement
SMS	Scientific Methods Scale
SOTP	sex offender treatment programs
TAU	treatment as usual
WOVM	Whole-of-Vehicle-Marking
WSIPP	Washington State Institute for Public Policy
WTP	willingness to pay
YSC	youth service counsellor



Introduction and overview of cost-benefit analysis

In recent years, policymakers have been increasingly influenced by cost-benefit analysis (CBA) when allocating resources to crime reduction and criminal justice programs. Public policy analysts have applied CBA for a number of years to assess environmental, social and economic projects. Although a small number of researchers applied CBA within the context of the criminal justice system and crime prevention field during the 1980s and 1990s, it has been increasingly embraced in the last decade (Aos 2002; Cohen 2000; Farrell, Bowers & Johnson 2005; McDougall et al. 2003).

Locally, the Australian Institute of Criminology (AIC) has estimated that the cost of crime in Australia for 2005 was \$35.8b (or 4.5% of national GDP; Rollings 2005), representing a substantial loss to the Australian economy. Given the difficulty of attaching a monetary figure to the lost productivity of those individuals committing the crime as well as their victims, and the intangible costs such as fear of crime, pain, suffering and lost quality of life, this figure is likely to be an underestimate.

The substantial costs of crime and the limited resources available for crime prevention programs provides a compelling argument for a systematic approach for allocating scarce public resources among competing programs or policies on the basis of CBA. CBA is a tool that enables a comparison of the advantages and disadvantages of undertaking a

particular program or policy as opposed to another course of action, including doing nothing at all, and applying monetary values to these advantages and disadvantages (Aos 2002). More specifically, CBA provides a tool for program analysts and policymakers to evaluate crime prevention and criminal justice programs from an economic perspective in order to guide decisions regarding whether to modify, expand or terminate projects (Dhiri & Brand 1999).

Conducting a CBA has three main advantages:

- it controls for differences in currencies and comparative monetary value;
- it controls for differences in time periods; and
- it presents the single monetary benefits that were provided for each unit of input invested in the program (Welsh & Farrington 2001).

Although, CBA is often (erroneously) used interchangeably with program evaluation, CBA does not determine if the project works (ie the program's success in achieving a specific outcome; Aos 2002), rather, it provides a quantification of expected costs and benefits.

This report outlines what CBA is, and where and how it can be used. It further provides a systematic review of several CBAs that have been applied to criminal justice or crime prevention programs. Finally, the report provides a tool to assess the effectiveness of CBA in evaluating the program in monetary terms.

Difference between cost-effectiveness and cost-benefit analysis

Although a number of approaches exist for evaluating programs in economic terms, two of the most commonly used techniques are cost-effectiveness analysis (CEA) and CBA (Dhiri & Brand 1999). The main difference between the two is that CEA considers only the costs as they are expressed in monetary terms, while CBA goes one step further to quantify the monetary benefits of the outcome (Cohen 2000). Cost-saving analysis and cost analysis are two other approaches used to analyse program costs and benefits (AIC 2003).

Cost-effectiveness is expressed in terms of the inputs required to produce a certain outcome or output (Dhiri & Brand 1999). CEA compares different cost streams to produce a specific outcome or outcomes. The project that produces the preferred outcome, or set of outcomes, at the lowest cost is considered the most desirable. For example, if the objective is determining the cost per violent assault avoided (cost per outcome), the calculation of a CEA requires quantification of the cost and volume of inputs required to result in the identified outcome; in this case, the avoidance of one violent assault. For comparisons to occur between alternate programs and interventions, they must share a common outcome and the inputs must be measured on a common basis.

CBA extends CEA by attaching monetary values to the outcomes of a program. After the cost of inputs and outcome benefits have been quantified in monetary terms, a comparison of alternate interventions can be made. The result is expressed in terms of a benefit/cost ratio which is calculated by dividing the monetary value of outcomes by the input costs. This ratio indicates the benefit received for every dollar contributed. For example, the benefit/cost ratio of 1.35:1 for a burglary prevention program indicates that for every dollar spent on this program, \$1.35 of benefits is received (eg by the avoidance of future burglaries). Net economic benefit is also calculated in a CBA by reducing the sum of benefits by the sum of input costs. The higher the cost-benefit ratio and net economic benefit associated with the project, the more desirable the project.

Two major types of CBA, *ex-ante CBA* and *post-ante CBA* have been identified to assist performance analysts and policymakers in resource allocation decision making (Chisholm 2000). *Ex-ante CBA* is conducted prior to the program's commencement and can assist governments in their cost-effectiveness strategies and selection of competing programs. *Post-ante CBA* is conducted after a project has commenced and has a limited effect upon government decisions for future resource allocations as capital costs have already been expended. However, *ex-ante CBA* can provide greater information regarding the performance of a program and subsequent benefits can be more accurately determined to inform further investment or whether a similar program should be funded.

The third approach, cost-saving analysis, is limited to the costs and benefits recognised by a program's funding organisation (which is often a government agency). Only the costs to the funding organisation are included and the benefits are expressed as dollars. This type of analysis is used to determine whether a program funded by the public 'pays for itself' — allowing a program to be justified in financial terms as well as on the basis of services provided (AIC 2003).

The fourth approach to cost analysis involves no measurement of benefits, although it can be useful to decision makers when recognising factors that need to be taken into account for replicating a program elsewhere or for informing budget projections (AIC 2003).

Application to crime prevention field

The growing use of CBA in the criminal justice arena has been driven by the increased public demand for transparency and accountability of government-funded projects. Use of CBA has been facilitated by improved data availability and techniques for quantifying the benefits and cost of intervention programs (Cohen 2000). Because it has become increasingly necessary for crime reduction and social intervention programs to be defensible on financial grounds, CBA has been used to demonstrate the value of programs and to justify allocation of

resources to these programs. As such, the use of CBA by crime prevention agencies has been a more proactive, rather than reactive, strategy.

A commonly cited social intervention program that indirectly provided crime reduction benefits is the HighScope Perry Preschool program (commonly referred to as the Perry Preschool program). This program also included a rigorous CBA and the conclusion that for every dollar invested in early care and education, the study shows, the return to society is more than \$16 (Schweinhart et al. 2005) has proven to be effective in communicating the cost-effectiveness of this prevention programs for policymakers. The Perry Preschool program was started in Michigan in 1962 and is one of the most commonly illustrated and rigorously evaluated CBA prevention programs. Perry Preschool was a program for preschool children and their parents who were living in poverty; it was designed to make significant life-course developmental gains in disadvantaged children. Of the \$16 return for each dollar spent, \$12.90 per dollar savings went directly to the public and the remaining \$3.90 went to the program participant. One of the numerous benefits the study produced was a significant reduction in crime among study participants including a reduction in overall arrests for violent crimes, property crimes and drug crimes, as well as a reduction in subsequent prison or jail sentences over study participants' lifetimes up to the age of 40 years. The crime-related effects, that is, the separate effects of criminal justice cases and victim costs that were avoided through this program's intervention, revealed a benefit-cost ratio to the public of \$11.31:1 for every dollar spent on the program; society and potential victims were expected to save \$11.31 in future avoidance costs (Schweinhart et al. 2005). Of the public return, assessed when participants were aged 40 years, 88 percent resulted from savings in crime-related costs.

Another frequently cited prevention program, which has been the subject of CBA, is the Prenatal/Early Infancy Project, more generally known as the Elmira program. Treatment effects of the Elmira program, now known as the Nurse-Family Partnership (NFP), have been examined during 30 years of program research (Olds 2008). This intervention program involved pre-natal and post-natal visits by nurses to economically disadvantaged first-time mothers, with

the greatest benefit resulting from targeting high-risk families. Aos et al. (2004), as part of a meta-analysis of early intervention programs, conducted a CBA on the NFP intervention and demonstrated a cost saving to governments of \$2.88 for every dollar spent in the form of reduced public assistance outcomes, criminality, child abuse and neglect, teen pregnancy, substance abuse and public assistance outcomes. Moreover, Greenwood et al. (1996) demonstrated a positive cost saving to governments of \$4.00 for every dollar spent.

The Elmira and Perry Preschool studies demonstrate that often the prevention of crime as a cost-benefit is but one of a number of outcomes that might be generated. Despite the significance of the outcome for the participants and its implication for the criminal justice sector and crime prevention, crime prevention may be a secondary objective for interventions that have been generated by non-criminal justice sectors such as health, welfare or early childhood development. When assessing CBAs in the criminal justice field, it is clear that there are substantial disparities in quality of assessment and it is therefore important that a proper assessment incorporate other forms of analysis, as they may provide some of the best support for crime reduction strategies.

The UK Home Office required all programs funded by the Crime Reduction Programme to incorporate CEA to ensure adequate assessment of the initiatives seeking funding. Dhiri and Brand (1999) developed guidelines on behalf of the Home Office for the standardisation of CBA analyses and CEA to improve comparability between proposals. In addition, the Home Office encourages researchers to include information on costs and benefits in their evaluations and they provided guidance to support this practice (Legg & Powell 2000). Similarly, the Canadian Government, through Canada's National Crime Prevention Centre (NCPC), provides guidance on performing economic evaluations of crime prevention programs and provides a manual for evaluators (Hornick, Paetsch & Bertrand 2000; Welsh 2007).

The Washington State Institute for Public Policy (WSIPP) has undertaken numerous CBA and systematic comparisons of crime prevention programs to inform policy decisions. The projects involve quantifying all tangible costs and benefits, and representing these from the perspective of both

the taxpayer and the crime victim. The National Institute of Justice, within the US Department of Justice, has also emphasised their role in performing outcome and cost-benefit evaluations of criminal justice programs (National Institute of Justice 2008). Estimating the costs of crime and analyses of CBAs has also been undertaken by the New Zealand Ministry of Justice (MacCallum 1997) and the AIC (Chisholm 2000).

Informing crime prevention policy

Significant commitment exists across public service agencies to ensure best available evidence, knowledge and research are used to enhance the nature, distribution, effectiveness, efficiency and quality of public services (Nutley, Walter & Davies 2007). Evidence-based policy has been defined as a method that

helps people make well informed decisions about policies, programmes and projects by putting the best available evidence from research at the heart of policy development and implementation (Davies 2004: 3).

Evidence-informed policy has been implemented in the United Kingdom and in the United States, however, a critique of evidence-based policy is that research and other evidence can be used selectively to back an ideological argument and support a pre-existing plan (Nutley, Walter & Davies 2007). Nutley, Walter and Davies (2007) have noted that critiques regarding the use of research in policy are not generally about *whether* research has been used, but rather *how* it has been used. Despite the variable quality of CBAs, there is a growing demand by governments in developed countries to incorporate economic analyses into criminal justice funding proposals for consideration by policymakers (Swaray 2006). This is reflected in the Council of Australian Governments' commitment and guidance for the use of CBA as part of a range of quantitative approaches to evaluate regulatory impact (COAG 2007).

For example, the Australian Government Department of Finance and Deregulation has also highlighted the importance of an evidenced-based policy with all regulatory proposals 'made by the Australian Government and its agencies that are likely to have a regulatory impact on business or the not-for-profit

sector' requiring a Regulation Impact Statement (RIS; DoFR 2010: 8). An RIS includes a brief analysis of the proposed costs and benefits of each option, followed by a recommendation. Guidance has been provided for conducting more comprehensive CBAs to improve policy decisions, as well as allow post-evaluation of a project or program, although application of this framework to rigorously assess criminal justice interventions has thus far been limited (DoFR representative personal communication April 2009).

Although an expectation exists that policymakers will place significant weight upon program effectiveness and cost-efficiency when allocating taxpayer dollars among alternative programs, this is not always the case. Examples of this include the Scared Straight prison deterrence program implemented in the United States, which various studies have demonstrated had no effect upon crime generally (Finckenauer & Gavin 1999; Petrosino, Turpin-Petrosino & Finckenauer 2000) and promoted recidivism in some cases. An evaluation of the San Quentin Squires Program, another implemented 'scared straight' program, found 81 percent of individuals who participated in the Scared Straight intervention program were arrested during the 12 month follow-up period compared with 67 percent of the control group (Lewis 1983). In addition, Aos et al. (2001) noted that individuals who had completed Scared Straight-type programs were, on average, 13 percent more likely to be arrested prior to the program's completion than juvenile offenders who had undertaken regular juvenile processing. Similarly, the widely implemented DARE program (Drug Abuse Resistance Education) aimed at reducing substance abuse and crime in school aged children in the United States has been demonstrated to have limited effect at best on later drug use and criminal behaviour (Ennett et al. 1994).

Another example includes analyses of the Reducing Burglary Initiative (RBI) implemented in the United Kingdom by the Home Secretary to motivate burglary reduction activity in domestic premises in communities identified as most at risk. The RBI involved the funding of 247 crime reduction projects over a three year period. The program was assessed by a number of parties including Hamilton-Smith (2004) and Kodz and Pease (2003) on behalf of the Home Office, as well as Hope (2004) as an

independent evaluator. Interestingly, the same study with identical data produced different conclusions and policy recommendations. Kodz and Pease (cited in Hamilton-Smith 2004) concluded the RBI produced a considerable quantity of crime reduction activity, estimating the net reduction in domestic burglary of more than 20 percent. According to Kodz and Pease (2003), 13 out of 20 projects were successful in domestic burglary reduction; while Hope (2004) reported only seven of the 20 projects achieved a significant impact on burglary in their target area after a re-analysis of the data. These seven also included one project that appeared to cause an increase in burglary in its targeted area. The primary difference between the evaluation methods was Hope (2004) incorporated a time-series method to isolate the proportion of change resulting from the project, rather than other factors occurring at the same time. Hope (2004) raised concerns regarding evaluation of programs by the same people who initiated and managed the development of the project, due to the temptation to validate the project as a result of policy pressures.

Steps in conducting a cost-benefit analysis

Six main steps have been identified for the successful completion of an economic analysis (Barnett 1993) which are applied to CBA. These include:

- defining the scope of the analysis;
- obtaining estimates of program effects;
- quantifying the monetary costs and benefits;
- calculation of present value and assessment of profitability;
- identification of the distribution of costs and benefits; and
- testing the riskiness of the conclusions via a sensitivity analysis.

Defining the scope

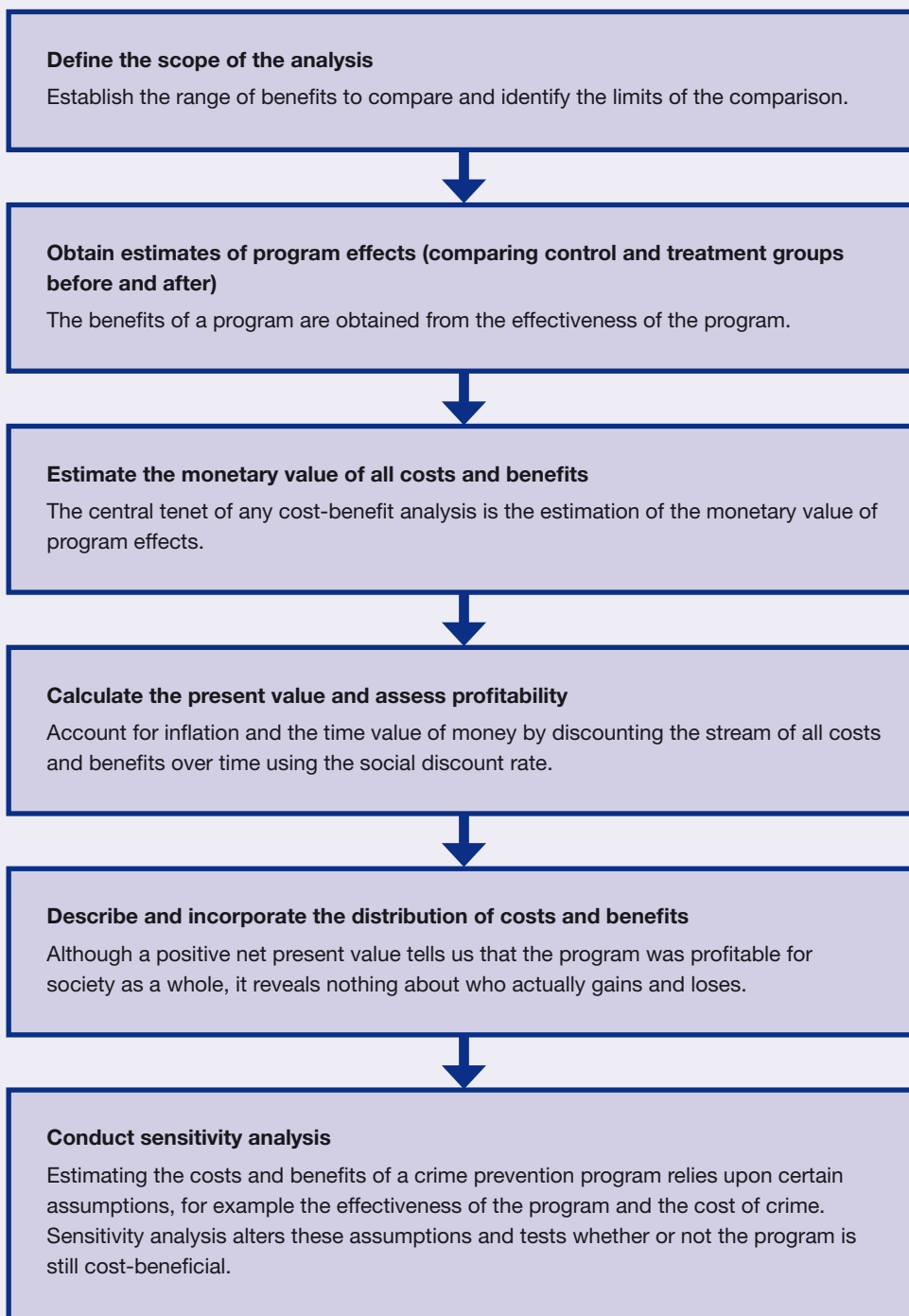
Defining the scope of the analysis determines the viewpoint the economic analysis will take. The

majority of criminal justice CBAs take the viewpoint of the government/taxpayer and potential victim of crime. The determination of the viewpoint is important for evaluation of the project, especially when the program is being funded by public money. If benefits and costs are assessed from the public's viewpoint then the benefits and the costs must be those that the public will either receive or pay (Welsh & Farrington 2000b).

For a CBA to be completed, the fundamental question *does the program work?* needs to be answered. This requires estimating the likely outcome in the absence of the program and eliminating other external factors that could affect the program's outcome (Welsh & Farrington 2000b). Weimer and Friedman (1979: 264) advise that prior to conducting a CBA, the program's design should be either 'experimental or strong quasi-experimental design'. For determining a program's effectiveness, the randomised experiment is the most persuasive method for measuring reductions in crime outcomes, given its high internal validity (Farrington 1983).

The main feature of randomised experiments is that the experimental group and the control group are identical for all possible exogenous variables that would be expected to affect the outcome. Randomisation of individuals between control and experimental groups provides greater assurance that the observed effects were caused by the program (Farrington 2003). Random assignment of units to either control or experimental groups can only occur with sufficient sample size. In general, 50 units are required in each category for randomised experiments (Farrington 1997). Non-randomised experiments and experiments that only examine before and after outcomes provide less convincing evidence of program effects (Welsh & Farrington 2000a). However, it is also not always feasible to have a randomised sample in the 'real world', therefore, quasi-experimental with comparison group are more common. Random assignment of experimental participants may be impractical, unethical, or impossible leading researchers to adopt a quasi-experimental methodology. For example, a randomised experiment may not be possible due to ethical concerns whereby excluding one group from treatment may, in an extreme situation, cause death/permanent damage.

Figure 1 Steps for completing a cost-benefit analysis



Source: Barnett 1993

Determining benefits

Estimating and assigning monetary values to the benefits arising from a program is commonly regarded as being the most difficult step of CBA. It is important to note that the terms *cost* and *benefit* are inversely related, in that the costs of a crime are the same as the benefits received from reduction of that crime. For benefits to be calculated, those who bear the cost of crime need to be identified. The costs and benefits of crime reduction accrue to different parties including crime victims, potential crime victims, taxpayers and government agencies (Cohen 2000). Cohen (2000) provides a comprehensive listing of the costs of crime and who bears these costs.

Although the costs of crime incurred by society can never be completely incorporated due to all the tangible (concrete), intangible (less definable) and 'knock on' effects that must be taken into consideration (Dhiri & Brand 1999), recent CBAs have endeavoured to go beyond the tangible costs of crime to incorporate intangible costs. One example of knock on or subsequent effects would be a program intervention aimed at reducing recidivism through the provision of drug treatment that also positively affects a participant's earning potential by facilitating access to a stable work environment, better relationships with family and friends and an improved housing situation.

Tangible and intangible costs

Costs incurred by the victim are classified into two broad categories—tangible and intangible. Although tangible costs including medical fees, lost wages and police and prison expenditures are seemingly easy to quantify, this is not necessarily so. There is no standard accounting system that incorporates all the cost of crime to victims (Cohen 2000). The United States *National Crime Victimization Survey* conducted biannually by the Bureau of Justice, provides a good guide to a victim's out-of-pocket costs via responses to personal and household victimisation. This information is obtained by interviewing households in the United States and enquiring about costs relating to victimisations. While the survey provides a good guide, several

methodological limitations result in underestimation of the tangible cost of crime. For example, the survey only enquires about the cost of victimisations in the past six months. Consequently, long-term costs such as medical expenses may be excluded. In addition, although mental health-related costs can account for a sizeable proportion of victimisation costs, they have historically been excluded from the survey (Cohen & Miller 1998), a trend which appears to have continued in the 2008 iteration (Groves & Cork 2008).

Intangible costs include psychological and emotional costs, such as the cost of pain, suffering and lost quality of life resulting from the victimisation and are more subjective than tangible costs (Cohen et al. 2004). Intangible costs in early criminal justice CBAs were excluded due to the lack of data available to estimate these costs and initial resistance to their inclusion (Farrell, Bowers & Johnson 2005). Farrell, Bowers and Johnson (2005) demonstrate that although the monetary costs of a car theft may be quite high, the intangible costs are considered to be low and relatively short lived. Conversely, the intangible costs to the victim of sexual abuse are not only significant but also enduring. For example, higher rates in adulthood depression, anxiety, alcohol and drug abuse, eating disorders and post traumatic stress disorder have been reported (Mullen & Fleming 1998). Although it can be argued that placing a dollar figure on the intangible costs of violent crimes is cold and detached, the alternative of not recognising these costs is much worse.

Intangible costs are legitimate costs to incorporate into CBAs in addition to the direct tangible costs incurred. In general, estimates of intangible costs are relatively conservative, with assessors likely to understate these costs (Wise et al. 2005). Although estimation of these costs can be difficult, a number of methods have been established to place monetary values on intangible losses. One of the earliest methods for estimating the intangible costs of individual crimes was based on the portion of jury awards compensating for pain, suffering and loss of quality of life (Cohen 1988; Miller, Cohen & Wiersema 1996). This approach was controversial because jury awards in the United States, for example, are perceived as unpredictable and unreasonably high, although it is argued jury awards become predictable when examined as part of a large sample (Cohen 2000).

Another method of measuring the intangible costs of crime is the ex-ante concept of *willingness to pay* (WTP), also known as 'contingent valuation'. WTP measures the amount individuals are prepared to pay to reduce the risk of crime. This can be a more appropriate measure than the ex-post concept of jury awards (Cohen 2000). WTP has been extensively used in environmental economics to value goods that do not have an available market (Cohen et al. 2004). One of the earliest examples of application of WTP to criminal justice was work by Cook and Ludwig (2000), who estimated individuals' WTP for a prevention program to avoid gun violence. They found that in general, the aggregate amount individuals are willing to pay for reduced crime is greater than the compensation for pain and suffering contained within jury verdicts. Cohen et al. (2004) estimated individuals' WTP to reduce a range of specific crimes was 1.5 to 10 times more than previous jury verdicts had estimated (Miller, Cohen & Weirsema 1996). Cook and Ludwig (2000) attributed the higher value to the individuals' willingness to not only prevent crime victimisation to themselves, but to others as well.

Expenditure is also incurred by society as a whole to reduce or avoid potential future victimisations. Individuals take preventive action (eg catching a taxi after dark instead of walking home) and/or defensive actions (fitting burglar alarms) to reduce the likelihood of victimisation. Individuals also pay insurance premiums to limit the consequences of potential victimisations (Dhiri & Brand 1999). The burdens of crime preventive/defensive expenditure, as well as reduced quality of life, fall disproportionately on individuals who have experienced prior victimisation (Dhiri & Brand 1999).

Taxpayers also bear a substantial portion of the costs of crime, through funding for:

- crime prevention programs;
- costs prior to conviction, including costs of police investigations;
- court/judicial costs; and
- corrective costs, such as prisons, rehabilitation and probation services.

The prevention, or reduction, of crime results in savings to taxpayers as a whole, who would otherwise be paying for the progression of offenders through the criminal justice system. Society, in

general, also benefits from crime reduction because members avoid being the victims of future crime. Some CBA also quantify the benefits from the offender's perspective, that is, improvement in career and life-course gains. They may assess the benefits to the offender's family of their family member avoiding future criminal behaviour, although this element is rarely included in the CBA (Aos 2002).

Moreover, changes in an offender's non crime-related benefits due to a crime program can also be important, though they may be less frequently measured. These measures can include improvements in an offender's education, employment, substance abuse, health, relationships and family factors. A comprehensive CBA will attempt to identify, measure and analyse these benefits in dollar terms.

Although the majority of CBA have a relatively short-term follow-up period to observe the outcomes of the program, a more comprehensive analysis would include the long-term effects of the program, as many results may not be evident for a number of years. This focus on long-term effects is important as programs aimed at reducing/preventing crime can absorb substantial long-term economic resources, such as prisons, rehabilitation, probation services and detention facilities (Aos 2002). A long-term analysis allows different programs to be compared to ensure resources are best spent.

Two areas of importance in situational crime prevention are displacement of crime and diffusion of benefits. Displacement refers to the unintended shift of crime to other locations, times or types in response to crime prevention strategies (Barr & Pease 1990). Reppetto (1974) distinguished six different types of displacement:

- temporal (change in time);
- tactical (change in method);
- target (change in victim);
- territorial (change in place);
- type of crime or functional (change in crime type); and
- perpetrator (apprehended offenders are replaced by new ones).

Conversely, diffusion of benefits refers to the unintended reduction of crime in other areas or non-targeted types of crime following the

preventative scheme (Clarke & Weisburd 1994). These two issues are important to integrate into the CBA to ensure all outcomes of the preventive action are measured.

After the perspective has been established, the dollar value of the benefits is calculated. Clearly, taxpayers and crime victims benefit each time a criminal act is avoided, but by how much? The lack of standardisation in calculating the approximate gain each time a criminal act is avoided is a recurring problem in performing CBAs (Welsh & Farrington 2000b). The need for standardisation of unit monetary cost for different crime types is crucial for the comparison of alternative crime prevention programs. Miller, Cohen and Wiersema (1996) developed estimates of the costs of crime for different types of violent and property crime, incorporating property damage and loss, medical care, mental health care, victim and social services, productivity and police services. Intangible costs, such as pain, suffering and lost quality of life, were also included in the estimates. The Washington State Institute, a leader in CBA for criminal justice, uses estimates of per unit victim costs developed by Miller, Cohen and Wiersema (1996) to perform cost-benefit analyses. The Home Office (Dubourg, Hamed & Thorns 2005) has also developed victim cost per unit for a number of different crime types to be used in CBA.

Determining costs

This step involves estimating the cost to conduct the program. In undertaking CBA, analysts emphasise the importance of only including costs and benefits that vary with the decision being made (Cohen 2000). This requires an understanding of fixed costs versus marginal and average costs. Fixed costs do not vary with the volume of output, and are often referred to as 'sunk' costs, as the costs will be incurred regardless of whether the program is implemented or not. For example, the annual expenses incurred to maintain a jail (ie depreciation of buildings, salaries for jail wardens) will be incurred regardless of the level of occupancy. However, once the prison reaches a certain capacity of inmates, the expenses might increase due to policy decisions resulting in prison expansion, additional employment of staff etc.

Unless fixed costs change in relation to policy decisions, these costs should be put aside for the purpose of CBA.

An important distinction needs to be made between marginal and average costs. Marginal (or variable) costs vary with each additional unit of output, that is, feeding an incarcerated individual, medical care etc, while average costs are calculated by dividing the total costs by the total volume in a certain period of time. Therefore, average costs include both fixed and marginal costs.

Large-scale capital costs

The inclusion of large-scale capital costs, such as acquiring asset or purchasing equipment, can affect the CBA. For example, the inclusion of (possibly) large-scale capital costs to calculate the economic efficiency of a program that is implemented for a relatively short period of time could result in the costs eclipsing the benefits received in this same timeframe. Welsh and Farrington (2000a) recommend spreading the capital costs over the life of the project to obtain a more accurate picture of the costs of the program.

Although a simple solution to calculating the bottom line is to deduct the costs from the benefits, this could result in an incorrect conclusion as costs and benefits may be distributed unevenly over a number of years.

Discounting value

For future costs and benefits to be compared over time, these monetary amounts need to be discounted back to their present values (Dhiri & Brand 1999). For example, although the cost of a program aimed at reducing recidivism may be incurred now, benefits of lower recidivism, such as the reduced costs of processing these individuals through the criminal justice system, may continue to yield benefits for a number of years after the program has ended. If the project has funding commitments that will continue into the future these future costs must be adjusted in two stages to account for differences in the value of money over time. First, the discounting of future monetary benefits is required to account for the preference for immediacy and the time value of money. The time value of money describes the

concept that one dollar today is worth more than one dollar tomorrow as you can invest the dollar today and earn interest on it (Gramlich 1990). Conversely, incurring a cost tomorrow is preferable to incurring the cost today. Second, the effects of inflation must be removed in order to translate 'nominal dollars' from each year into dollars of equal purchasing power, or 'real dollars' (Barnett 1993).

After completion of the abovementioned steps, the total discounted present value of benefits, minus the total discounted present value of costs, is calculated to produce the assessment of net present value. The net present value is often used for comparison between alternative programs, in conjunction with the cost-benefit ratio.

Sensitivity analysis

The final stage of the CBA is testing the risks associated with the conclusions. Due to the uncertainty of many of the inputs and assumptions in a CBA, it is important to test how sensitive the conclusions are to changes in key inputs. One method for dealing with this uncertainty is to perform a sensitivity analysis, which is considered the most useful tool for representing risk. A sensitivity analysis varies assumptions and external influencing factors to consider alternative outcomes (Dhiri & Brand 1999). A sensitivity analysis, performed for CBAs incurring costs and receiving benefits over a number of years, can be used to compare outcomes by varying the discount rate. This is reasonable considering that evidence suggests the cost-benefit ratio can be more sensitive to changes occurring within the timeframe used to calculate returns and costs (Farrell, Bowers & Johnson 2005).

Limitations of cost-benefit analysis

Cohen (2000) has identified a number of issues that affect CBA. These include:

- the potential for misuse of CBA;
- uncertainty;
- treatment of future benefits and costs;
- issues of fairness and equity; and
- public perception of the risk of crime.

Roman (2004) has also highlighted additional concerns including lack of a well-developed empirical method, magnification of small errors from sample size and selection effects, and/or the exclusion of critical information.

The misuse of CBA can occur through carelessness, inexperience and/or deception. Dilulio (1996) has attributed the sometimes substantial disparity in the quality of criminal justice CBAs to the broad range of backgrounds of the practitioners who have entered the field. According to Dilulio (1996: 3), generally these individuals lack experience in quantitative and formal modelling skills to not only provide new ideas to the debate but also to provide 'analytically compelling answers to methodologically complicated questions' on quantifying crime prevention and criminal justice.

Cohen (2000) has noted the results of individual CBA can be criticised on the basis that although economic analyses are often perceived as objective and accurate, this can be misleading. Any particular CBA is only as accurate as the figures and assumptions it is based on. The assumption that \$X spent results in \$Y saved overlooks the subjectivity behind the calculations. Bias in what benefits are included in the analysis and the calculations behind these figures can impact on the analysis and may even turn a CBA from being economically unfavourable to favourable. Moreover, projections of long-term benefits based on intermediate outcomes will be subjective. For example, forecasting improved future employment prospects based on higher educational attainment as a result of program participation could result in significant overestimates or underestimates of program benefits due to small differences compounded over predicted years. The inclusion and estimation of intangible costs can have a substantial effect upon the results of the CBA and create difficulties in comparing alternative crime reduction programs. For this reason, identification of the assumptions and the valuation of these assumptions must be fully transparent to enable comparison between programs and improve the credibility of analyses.

Considerable uncertainty can arise surrounding the costs and benefits of a program due to a reliance on certain assumptions. Frequently, implementation of the program will be based on an evaluation of a

program which has already been implemented. However, differences between programs, such as demographic differences between the offending or treatment populations, the punishment or treatment protocols, program personnel and the time periods, can result in varying outcomes (Cohen 2000). A change to any one of these elements could affect the effectiveness or the cost of a program. Performing a sensitivity analysis, which varies the inputs used in the analysis to examine the impact of the variation on the outcomes, is one method for dealing with this uncertainty.

Another hurdle to effectively comparing alternative programs arises when the benefits and costs of a program may not occur for a number of years. This is generally seen when an evaluated program has large upfront costs and small benefits incurred over a long time period of time. This can arise because of a lack of consensus regarding the discount rate applied. Cohen (2000) observed that the majority of criminal justice CBAs utilise a discount rate between two to three percent. Although the Home Office recommended a standard discount rate of six percent in 1999 and 2000 (Dhiri & Brand 1999; Legg & Powell 2000), this rate was revised to 3.5 percent in 2003 (HM Treasury 2003) and is 3.5 percent as at 2010 (HM Treasury 2010). The Office of Management and Budget (2009) in the United States periodically updates the discount rates for CEA. The revised discount rate, in 2009, ranges from 0.9 percent (3 year cash flow projection) to 2.7 percent for 30 year cash flows. In Australia, the Department of Finance and Deregulation publishes recommended discount rates to ensure consistency; currently recommending a discount rate of seven percent, with sensitivity analysis at three percent and 11 percent. The variation in discount rate can have significant impact on the outcomes of the CBA for long-term projects, as the higher the discount rate, the lower the present value of future cash flows. However, as programs usually incur costs and benefits in time periods close together and fiscal assessments of programs are primarily evaluated over a short period of time, the effect of discount rates is, in a practical context, minimal.

CBA can also reflect social inequities. Cohen (2000) highlights this issue through the following example. If a program evaluator is measuring the loss of wages for a crime victim and these crime victims earn below average income, the benefits of the program will be estimated downwards based on a lower income. In addition, an individual's WTP for a program to reduce crime would be dependent on the individual's wealth. Therefore, to avoid discriminating against poorer victims, the analyst needs to quantify the costs and benefits from the perspective of the 'typical person', rather than from the perspective of a specific crime victim (Cohen 2000).

A discrepancy can exist between the public's perception of the likelihood of crime victimisation and the actual risk of victimisation. This is particularly apparent in data examining the public's concern of growing crime amidst declining crime rates (Cohen 2000). This discrepancy can affect the individual's WTP, resulting in the individual being prepared to pay a much higher amount to reduce perceived victimisation of crime than if they were aware of the true likelihood of being a crime victim. Cohen et al. (2004) argues WTP incorporates fear of crime and public wellbeing based on actual levels of fear and concern, thus measuring the actual amounts individuals would be willing to pay to reduce these perceived risks.

Welsh and Farrington (1999), after conducting an extensive analysis of the application of CBA in the criminal justice field, concluded that the quality of empirical CBAs varied widely. Roman and Farrell (2002) suggested four explanations for the lack of empirical CBAs conducted. First, rigorous quantitative research into crime prevention is relatively new. Second, many policymakers and researchers believe that if the program has effective and positive outcomes, the benefits outweigh the costs, no matter what the cost. Third, it can be expensive and time consuming to complete a comprehensive CBA. Finally, even the most comprehensive CBA can often be picked apart due to necessary assumptions, resulting in researchers' reluctance to undergo CBA. It has also been suggested that few have the skills to complete a comprehensive CBA.

Evaluation of cost-benefit analysis

This section will briefly summarise a number of approaches to reviewing the efficacy of research findings, including findings from CBAs. These approaches include:

- meta-analyses;
- the Campbell Collaboration;
- the Maryland Scientific Methods Scale; and
- evidence rating system.

Next, a systematic review of a selection of relevant CBA case studies from both Australia and overseas will be undertaken (with summaries of these case studies provided in *Appendix A*).

The purpose of a systematic review is to use rigorous methods for searching, evaluating and integrating evidence from prior evaluation studies into an unbiased and comprehensive review (Farrington, Petrosino & Welsh 2001). An commonly cited example of a comprehensive systematic review is Welsh and Farrington's (2002) review of implementation of closed circuit television (CCTV) programs on crime reduction in the United Kingdom. This review combined 22 studies and concluded that the implementation of CCTV was most effective in reducing car theft in car parks. This systematic analysis provided policymakers with valuable information regarding the implementation of CCTV for crime reduction.

A meta-analysis involves quantitative or statistical analysis of the outcomes of completed research studies (Lipsey & Wilson 2001). As a meta-analysis involves the statistical review of research studies, this approach may not be appropriate if only a small number of studies are being evaluated, the type of studies differ substantially and for studies that differ in units of analysis (Welsh & Farrington 2005).

The Cochrane Collaboration is an international organisation that prepares, maintains and disseminates systematic reviews on the effectiveness of alternative healthcare interventions. Systematic reviews produced by the Cochrane Collaboration follow a standard structure to ensure consistency between the different reviews and are highly regarded for their systematic analyses of medical testing. The Campbell Collaboration has implemented a similar structure for conducting systematic reviews in education, crime and justice, and social welfare and is considered at the forefront of systematic reviews in the criminal justice field (Welsh & Farrington 2005). The Campbell Collaboration appointed a crime and justice steering committee (CJSC) in 2000 to oversee the preparation, maintenance and distribution of systematic reviews in research on the effectiveness of criminal justice interventions to prevent or reduce crime (Farrington, Petrosino & Welsh 2001). The framework developed by the Campbell Collaboration in addition to that developed by Welsh and Farrington (1999) and Perry

(2006) has been applied by the author to review the CBAs on crime prevention and criminal justice conducted here.

Comparable to the Campbell Collaboration framework, benefits are classified as monetary estimation of program effectiveness. Conversely, costs have been classified as program expenses such as prison, treatment and court costs.

Moreover, the Maryland Scientific Methods Scale (SMS; Sherman et al. 2002, 1997) has been employed as part of a systematic effort to categorise these study designs; similar to a number of reviews (Perry 2006; Welsh & Farrington 2000b). The Maryland SMS evaluates the reliability of the effects of the intervention program through the methodological quality of the experimental design (Perry 2006). An advantage of this scale is that it allows alternate study designs to be pooled together. This is important for CBA in the criminal justice field as the majority of the evaluated studies are either non-randomised experimental studies or observational cohort studies

(Perry 2006). The scale scores experimental design between 1 (the lowest) and 5 (the highest) as shown in Table 1.

Program evaluations rated as ‘1’ on the five point scale were not included in this systematic review as they were not convincing evidence of program effectiveness. Programs rated as a ‘2’ were also excluded because these studies have not included a comparison group, having only obtained pre- and post-test measures of target group. Studies that contained a comparison group that lacked comparability to the target group on pre-existing variables were also rated as a ‘2’. A research design that achieves level ‘3’ on the SMS is considered the minimum research design for drawing conclusions regarding the effectiveness of a crime prevention initiative (Farrington et al. 2006).

Therefore, studies scoring ‘3’, ‘4’ and ‘5’ on the Maryland SMS were evaluated as containing the most rigorous study design and were included in this review.

Table 1 Scientific Methods Scale

Level	Criteria
1	Correlation between a prevention program and measure of crime at one point in time
2	Measures of crime before and after the program, with no comparable control condition
3	Measures of crime before and after the program in experimental and comparable control condition
4	Measures of crime before and after the program in multiple experimental and control units, controlling for other variables that influence crime
5	Random assignment of program and control conditions to units

Source: Farrington et al. 2006

Table 2 Evidence rating system

1	Includes appropriately matched comparison group or randomised control design methodology
2	Pre-intervention (baseline) data available
3	Intermediate follow-up (ie collected up to 2 years after the intervention period)
4	Long-term follow-up (ie collected more than 2 years after the intervention period)
5	Representative sample of participants included in the evaluation
6	Low attrition at longitudinal follow-up (not more than 10% per data point) and attrition not systematic
7	Adequate statistical power for analyses
8	Reliable measures
9	Appropriate choice of outcome measures
10	Appropriate analytic approach
11	Number of evaluation design elements present

Source: Wise et al. 2005

The criteria for the author's assessment of the reviewed studies research design is based on Wise et al. (2005) 'Evidence Rating System', which was used to assess methodological quality of early childhood interventions. The 11 questions assess whether fundamental elements required for methodological quality of experiment are present. These are outlined in Table 2. Each question was either rated by the authors as 1–present or 0–not present. A score of '0' denoted information not reported, question not applicable or response to the question was no. A higher score represented greater methodological design quality and reduced the likelihood of bias or errors. As a CBA is based on the effectiveness of the study, the greater the methodological quality of the study, the greater the reliance that can be placed upon the cost-benefit calculations.

The quality assessment is designed to provide a level of reliability based on a high rating which indicates less bias in the experimental design. The Centre for Reviews and Dissemination (Khan et al. 2001) identify four sources of bias that may affect the results of the studies—selection bias, performance bias, measurement bias and attrition bias. Selection bias refers to systematic differences in characteristics between the experimental group and the control group. Randomisation of large numbers of participants and concealment of their allocation to different groups can reduce this bias. Performance bias relates to bias generated from differences in the delivery of the intervention. Ensuring practitioners deliver the treatment in a standardised manner and 'blinding' both the practitioners and the participants can reduce this bias. Measurement bias explains systematic differences in how outcomes are determined between the control group and the treatment group. This bias can be reduced by 'blinding' participants and outcome assessors. Finally, attrition bias relates to systematic differences between comparison groups in terms of withdrawals or exclusions of participants from the program sample. Inclusion of these participants and implementation of a sensitivity analysis can minimise this bias.

To ensure that the relevant methodological points are appraised for each economic evaluation, the Centre for Reviews and Dissemination (CRD) has adopted an 11 point quality checklist, based on Drummond et al. (1997), outlined in Table 3. The

studies reviewed by the author were assessed against the same 11 point quality checklist to determine the strengths and weaknesses of the CBAs.

Table 3 Eleven point quality checklist for assessing economic evaluations

1	Is there a well-defined question?
2	Is there a comprehensive description of alternatives?
3	Are all important and relevant costs and outcomes for each alternative identified?
4	Has effectiveness been established?
5	Are costs and outcomes measured accurately?
6	Are costs and outcomes valued credibly?
7	Are costs and outcomes adjusted for differential timing?
8	Is there an incremental analysis of costs and consequences?
9	Were sensitivity analyses conducted to investigate uncertainty in estimates of cost or consequences?
10	How far do study results include all issues of concern to users?
11	Are the results generalisable to the setting of interest in the review?

Source: Drummond et al. 1997

Selection criteria for studies included in this review

For programs to be included in this systematic review, five criteria had to be met (Károly et al. 1998). First, the evaluation had to have an experimental design, including a control group in addition to the treatment group. A rigorous experimental design provides greater assurance that the outcomes result from intervention rather than other extraneous variables. Second, the sample size had to be sufficiently large to allow the differences between the groups to be determined with statistical methods. Third, the sample attrition over time had to be low. Differences in attrition rates between groups can affect the comparability of the treatment and control group, and affect the size of the sample size. Fourth, the potential benefits had to be measured in the assessment and finally, there had to be sufficient follow-up to adequately measure the program's outcomes. An additional criterion included in this review was that only studies published from 2000 onwards were included in the analysis. As this field is expanding, this systematic review was unable to capture every evaluation, however, provides a snapshot of CBA studies and a tool for assessing these studies.



Case studies

Case study 1

Closing off opportunities for crime: An evaluation of alley-gating

Bowers, Johnson and Hirschfield 2004

A situational crime prevention program was implemented in an effort to reduce high levels of burglars gaining access to terraced properties through back alleyways in the City of Liverpool (United Kingdom). This program involved fitting gates to alleyways at the back of terraced properties to restrict access to residents and reduce opportunities for offenders to gain entry into properties. This is a common example of target hardening, whereby upgrading of physical security measures at vulnerable households reduces their susceptibility to criminal incidence. The project was implemented over a period of three and a half years, beginning in January 2000 through to June 2003. Due to the rolling implementation schedule, Bowers, Johnson and Hirschfield (2004) calculated how many burglaries occurred before and after installation of the gates and how many months elapsed in these two time periods. The main source of data included police-recorded burglaries.

Design and method

Bowers, Johnson and Hirschfield (2004) used an experimental-control, pre-/post-test design by comparing the before and after burglary rates for both treatment and matched comparison areas to determine the reduction in domestic burglaries as an outcome of program intervention. The situational intervention involved the installation of hardwearing lockable gates across the alleyways at the back of terraced (or other) properties in the City of Liverpool in Merseyside County, north-west England. This scheme would be relatively easy to apply to numerous suburbs in the United Kingdom due to the prevalence of long narrow alleyways at the rear of terraced and detached properties, and the ease with which the alleys could be restricted via the use of lockable gates.

The evaluation covered the three and a half year period of the multiple alley-gating scheme (1 January 2000 to 31 June 2003). The schemes were phased in over time due to the lengthy public consultation required and the time for manufacturing and fitting the individual gates. The pre-intervention period covered two years prior to the commencement of the scheme (1 January 1998 to 31 December 1999). The rolling implementation created an issue in examining the effectiveness of the scheme by only analysing pre-/post-test rates of burglary after the implementation of the gates due to the variation of pre- and post-periods across schemes. To overcome

this issue, Bowers, Johnson and Hirschfield (2004) divided the number of burglaries observed before and after the inception of the interventions by the amount of time that passed during these periods.

To examine the effectiveness of the scheme, a comparison group was established that did not have alley-gating implemented. To ensure that the comparison group was appropriate, the burglary rates in the selected comparison area were compared with the action area for the period pre-implementation. Bowers, Johnson and Hirschfield (2004) conducted a related sample t-test to ensure that the ratio of burglaries between the action area and the comparison area were similar over the two years prior to implementation (1998–99 and 1999–2000). The t-tests demonstrated that the comparison and the action areas followed a similar trend pre-implementation of the alley-gated scheme, validating the appropriateness of the comparison area. The statistical procedure to control extraneous variables via matching resulted in the rating of '3' on the SMS, determined by comparison with Table 1 Maryland SMS.

Bowers, Johnson and Hirschfield (2004) concluded that due to the comparison area and action area following a similar pattern in burglary trends in the absence of the scheme, the burglary rate in the action area would be likely to follow a similar trend to that exhibited in the comparison area. The authors multiplied the number of burglaries that occurred in the action area pre-implementation by the rate of change in the comparison area to provide an estimate of the number of burglaries expected in the action area. The number of burglaries prevented involved calculating the difference between the number of burglaries expected and the actual number observed in the action area. Due to the variation in timing of the implementation, this calculation was conducted for each scheme separately, in accordance with the relevant start date.

Due to the possibility of territorial displacement of crime and diffusion of benefits occurring following the implementation of the situational crime intervention, Bowers, Johnson and Hirschfield (2004) examined potential buffer zones surrounding the action area. Through the use of a geographical information system (GIS) a series of seven 200 metre buffer zones were generated to measure the incidence of territorial displacement and diffusion of benefits.

No follow-up data beyond 12 months was collected by the authors, resulting in the exclusion of future program benefits and costs. As the implemented gates have a lifetime of 10 years, there is a strong likelihood that future crime reduction, although not measured, will occur.

Results

Over the course of 12 months following the situational intervention, a 37.1 percent reduction in the risk of burglaries, relative to the comparison area, was observed in the action areas. Compared with the pre-intervention rate, an estimated 875 burglaries were prevented in the intervention period. Bowers, Johnson and Hirschfield (2004) recognised that a number of other crime reduction schemes and police department initiatives were also implemented during implementation period. Bowers, Johnson and Hirschfield (2004) conducted regression analyses and determined that the situational intervention explained 48–55 percent of variance in the burglary ratio for the alley-gated areas. Although, crime displacement occurred in the non-target buffer areas, there was an overall 13 percent reduction in the number of domestic burglaries in the non-targeted areas after 12 months, otherwise known as a diffusion of benefit. The rate of diffusion of benefit varied from eight percent to 40 percent depending on the distance of the buffer zone from the active area. The reduction could also reflect non program-related changing influences over the timeframe, although 16 percent of this diffusion benefit could be attributed to the intervention.

Bowers, Johnson and Hirschfield (2004) found that restriction to access via rear alleyways increased the number of burglaries for which access was gained at the front of the houses, or other entry points (tactical displacement). However, Bower and her colleagues determined that despite this displacement effect, a significant overall reduction in burglary was still achieved within the action area.

Economic analysis

A CBA was conducted. The total cost of the intervention was estimated at £2,094,302. This was made up of the cost of the gate installation, including the resident consultation process, and manufacture and installation of each gate. The total benefits were

estimated at £2,013,967 after three years. Benefits were calculated by estimating the number of burglaries prevented and attaching a monetary value to these avoided burglaries. The method of calculating the number of burglaries prevented involved calculating the difference between the number of burglaries expected, derived from the burglary trends in the comparison area, to the actual number observed in the action area. The number of prevented burglaries was then multiplied by the Home Office's calculation of the average cost to society of one burglary—£2,300. This cost included the criminal justice system cost and the value of goods stolen (Brand & Price 2000). Bower et al. (2004) considers the estimation of total benefits to be conservative, as other benefits in addition to burglaries avoided, were not included or monetised in this economic analysis. Dividing total benefits by total costs produced an undesirable cost-benefit ratio of 0.96. Therefore, for each £1 that was invested in this program, future victims of crime and taxpayers received £0.96 in benefits from burglaries prevented.

As the intervention was implemented over approximately two years and the majority of the gates at the end of year three had only been implemented for six months, an underestimation of the program's benefits is likely. The analyses that focused on action areas where the gates had been installed for 12 months or more produced a positive CBA of 1.86:1.

This assessment of Bower, Johnson and Hirschfield's (2004) crime reduction and CBA are relatively positive. From the available evidence, it appears that this sort of situational intervention can have a significant influence upon the reduction in crime and prove to be a cost-effective strategy. Although the study failed to provide follow-up program effects longer than 12 months, the cumulative program effects indicate that the study is likely to have underestimated the program effects. The CBA is likely to be an underestimate as the analyses did not measure or monetise a number of possible benefits resulting from the intervention. However, Bower et al. (2004) indicates that this CBA is rudimentary and the use of Home Office estimates to attach monetary values to the primary benefit—prevention of burglary costs—appears to be more important than identifying and monetising every possible benefit of the program. The use of a high quality experimental-control group design with pre-/post-measures to assess program

effects reduced the threats to internal validity. The combination of the large number of alley-gates involved in the evaluation and the attempt to examine displacement and diffusion effects increases confidence in the program effects. Overall, the author accepts the economic assessment of the situational intervention.

Case study 2

Are violent delinquents worth treating? A cost-benefit analysis

Caldwell, Vitacco and Van Rybroek 2006

Studies attributing the majority of serious and violent juvenile crimes to a small proportion of juvenile delinquents, as well as the likelihood that these same juvenile offenders will continue their serious offending into adulthood, have prompted research into, and implementation of, intensive treatment programs for serious and violent incarcerated delinquent boys. The intervention project was implemented over a period of 2.5 years at the Mendota Juvenile Treatment Center (MJTC) in Wisconsin, with an average 4.5 year follow-up. The MJTC program involved specialised intensive cognitive and behavioural mental health treatment. The 101 participants in the intervention program were matched on a treatment propensity score to a group of 101 juveniles who received 'treatment as usual' (TAU) in secured juvenile corrections setting (control group). Caldwell, Vitacco and Van Rybroek (2006) assessed treatment outcomes including the incidence of offending, severity of offending and the variation in institutional cost between the treatments. The source of outcome data for criminally charged offences, post-intervention, was collected from a database of public circuit court records. During admission into the MJTC, every youth received an in-depth multidisciplinary assessment. These assessments provided the data used to determine the equivalence (or matching) of the treatment and comparison groups. The treatment program studied was funded through state tax revenues.

Design and method

This experiment used a quasi experimental–control, pre-/post-test design by comparing the differences in frequency and severity of recidivism in serious and violent delinquent males between the treatment group and matched comparison group. The intensive treatment involved cognitive and behavioural mental health treatment, including aggression replacement training and reliance on the decompression treatment model to stop and redirect adverse behaviour and to engage highly disruptive participants in the program (Caldwell 1994). The ‘treatment’ group consisted of 101 delinquent males who received the majority of their treatment and rehabilitation services at MJTC and ceased treatment when their sentence finished. The ‘comparison’ group consisted of 101 delinquent males who, although receiving initial assessment at MJTC, undertook the majority of their treatment within secure juvenile corrections institutions (JCI). No screening was undertaken by MJTC staff to determine the allocation of juvenile delinquents into the control and treatment groups; however, individuals were generally transferred to MJTC because of their failure to adjust to the correctional institutional setting.

Although operated under the Department of Corrections, MJTC is located on the grounds of a state psychiatric hospital in the State of Wisconsin and staff were employed by the hospital. The program delivered by MJTC differed to treatment provided by JCIs in three ways. First, the treatment program consisted of single-bed rooms, while the conventional JCI accommodation consisted of up to 50 double-bunked beds in a cottage. Second, MJTC treatment was the responsibility of a psychiatric nurse manager, where as a JCI was typically run by experienced security staff. Finally, MJTC treatment had a strong focus on interpersonal processes, social skills acquisition and the development of social bonds.

The MJTC program’s intervention evaluation covered a 2.5 year period. The average follow-up period was 53 months (ranging from 14 to 92 months) and the authors collected outcome data on the number and type of criminally charged offenses post-intervention. Data sources included databases of public circuit court records, Wisconsin Department of Corrections database and MJTC in-depth multidisciplinary assessment.

To ensure the comparison group was appropriate, MJTC undertook multidisciplinary assessments of all

individuals in both the control group and the treatment group. This in-depth assessment included psycho-diagnostic interviews, academic and psychological testing, and gathering of relevant background information. Individuals in the comparison group were matched to corresponding individuals in the treatment group on the basis of a propensity score, consisting of 21 demographic, behavioural and clinical variables. The propensity score was implemented to reduce the effects associated with non-random assignment of participants between the treatment and control groups. The statistical procedure to control extraneous variables via matching has resulted in the rating of ‘3’ on the SMS, determined by comparison with Table 1 Maryland SMS.

Results

Over the course of 4.5 year follow-up, data on the impact of treatment on three outcome variables— all offenses, felony offenses and violent offenses—was collected. Participants in the matched treatment group averaged less than half the number of charged offenses in the follow-up period (1.09 versus 2.49 for the matched comparison group) and were convicted of less than one-third of the number of violent offenses (0.25 v. 0.85 for the comparison group). MJTC-treated individuals were also less likely to be convicted of a felony offence (0.48 versus 0.89 for the comparison group). For all outcome treatment variables, the effects were statistically significant. The difference in the rate and severity of recidivism between the control group and the treatment group was concluded to be due to the program’s implementation.

The average number of the days that the treatment participants spent in the MJTC program was 354 days. Due to the effectiveness of the MJTC program at improving the institutional adjustment of youth through increased treatment engagement, participants in the treatment group tended to lose fewer days to security interventions and progressed faster through MJTC than in the usual institutional correction. Consequently, the treatment group’s total time incarcerated was substantially less than the comparison group.

Economic analysis

A CBA was conducted. The total cost of the MJTC intervention for 101 delinquent males in the treatment

group was estimated at \$161,932 per youth. Conversely, the total costs of the 101 delinquent males in the TAU comparison group was estimated at \$154,918 per youth. Therefore, the mean marginal cost of treating a youth on MJTC was \$7,014. The cost of treatment in MJTC and the usual juvenile institution was calculated by multiplying the per diem cost by the number of days the youth resided in each setting.

Benefits were calculated for each individual by multiplying the number of charged offenses in each crime category by the criminal justice processing cost for that category. Only costs that were supported by taxpayers were included in the calculation. These costs were summed to determine the total criminal justice processing costs for that individual. This process was completed for all individuals in the comparison group and the MJTC treatment group during the 4.5 year follow-up period. The monetary value of the criminal justice processing costs were based on Cohen's (1998) criminal justice processing costs for each offense and adjusted to 2001 dollars. The average savings from avoided costs for the treatment condition were then calculated and compared for the final cost calculation.

The comparison TAU youth generated criminal justice costs of \$14,103 and prison costs of \$47,367, substantially more than the MJTC youth who generated criminal justice costs of \$5,927 and prison costs of \$5,153. More specifically, the benefits per MJTC-treated youth was made up of \$8,176 in avoided criminal justice processing costs plus \$42,214 in avoided prison costs. The total saved by the taxpayers on avoided criminal justice costs was estimated at \$50,390 per MJTC-treated youth over the 4.5 year follow-up period.

Dividing total benefits by total costs produced a desirable cost-benefit ratio of 7.18:1. For each \$1 that was invested in this program, taxpayers received \$7.18 in benefits from reduced recidivism.

Caldwell and his colleagues (2006) consider this estimation of total benefits to be conservative as only taxpayer-related costs of crime have been included in this calculation. Costs of crime including medical costs, lost wages, property losses and other victim costs had been excluded from this economic analysis and would dramatically increase the cost of these criminal offences, especially for violent and serious offences. As Caldwell, Vitacco and Van Rybroek (2006) concluded, the MJTC program

generally reduced more serious and violent offending and the inclusion of victim-borne costs from the economic analysis seems pertinent due to the focus of this program on reducing violent and serious recidivism among juvenile delinquents.

This review of Caldwell, Vitacco and Van Rybroek (2006) shows a significant reduction in recidivism and CBA that are relatively positive. From the available evidence, it appears that alternative treatments for serious and violent juvenile delinquent males can have a significant effect upon the reduction in future recidivism and prove to be cost-effective. Although the study failed to monetise criminal costs in addition to taxpayer expenses, the program effects indicate the study is likely to have underestimated the full cost-benefits resulting from the intervention. Further, Caldwell and colleagues (2006) have stated that due to the CBA being conducted from the perspective of costs directly paid from state tax revenues, a more conservative estimate of the program's cost-effectiveness is provided. The use of a high quality experimental-control group design with pre-/post-test measures to assess program effects reduced the threats to internal validity. The use of propensity scores to match the comparison group to treatment group increased confidence in the program effects. Overall, the economic assessment of the intervention program is accepted, although additional work addressing the issues discussed would be beneficial.

Case study 3

A short-run cost-benefit analysis of community-based interventions for juvenile offenders

Robertson, Grimes and Rogers 2001

Research indicating the increased effectiveness of community-based treatment, compared with regular probation, for juvenile delinquents provided a catalyst for this program's implementation. This program involved two treatment groups and one control group for a sample of youths referred to one of three Mississippi youth courts for delinquent

(criminal) activity or status offences (eg truancy, running away). The first treatment group included intensive supervision and monitoring (ISM), the second treatment group consisted of intensive outpatient counselling with cognitive-behavioural therapy (CBT) and the comparison group included youths that experienced standard probation or parole. Each of the three county youth courts participating in this study undertook one of the three intervention approaches. The intervention project was implemented over a period of six months, with a 12 month follow-up. The treatment outcomes assessed included the estimate of the change in justice system expenditures.

Design and method

This experiment used a quasi experimental–control, pre-/post-test design by comparing the differences in criminal justice system expenditure between the comparison group and two experimental treatment groups. The comparison group consisted of participants involved with the Madison County Youth Court and involved regular probation or parole intervention. These intervention procedures, regularly implemented with juvenile offenders by the Court, involve face-to-face meetings between the participant and a youth service counsellor (YSC) on a weekly to monthly schedule, as well as the YSC meeting with the participant's parent or guardian on a monthly or less frequent basis through telephone interviews or home visits. Each YSC maintained a caseload of between 80 to 100 juvenile offenders. The first treatment group involved intensive supervision and monitoring of juvenile offenders under the jurisdiction of the Lowndes County Youth Court. YSC's for the ISM group had comparatively smaller caseloads and more frequent involvement with youth and parents than their counterparts servicing the regular probation and parole intervention group. The CBT treatment group involved participants under the jurisdiction of the Forrest County Youth Court who were required to undertake intensive counselling sessions in addition to standard probation requirements and procedures. Professional counselling consisted of 60 hours of cognitive skills training classes, 24 hours of group therapy for the juveniles and 24 hours of group therapy for the parents.

A total of 293 participants initially entered the study. The control regular probation and parole group consisted of 99 participants, the ISM treatment group included 109 youths and the CBT treatment

group included 85 youths. Complete data records were only obtained for 153 participants. The attrition rate between the interventions varied with the regular probation and parole control group having 45 participants, the ISM treatment group 61 youths and the CBT treatment group 47 youths at the end of the respective six month intervention. Robertson, Grimes and Rogers (2001) stress this does not mean 140 (or 48%) of the participants failed to complete the treatment intervention, but instead, some were temporarily unavailable to complete the necessary assessments. The sample was restricted to youths who self-reported or exhibited substance use and youth court judges placed juveniles within the respective intervention, although participation in the study was voluntary and involved parental permission.

The duration of the CBT treatment, regular probation treatment and intensive supervision and monitoring covered a six month period. The follow-up period was 18 months.

To examine the effectiveness of the scheme, a comparison group was established that undertook TAU regular probation or parole. To ensure the comparison group was appropriate, the researchers undertook multidisciplinary assessments of all individuals in both the comparison group and the two treatment groups. This in-depth assessment included the Millon Adolescent Personality Inventory, Personal Experience Screening Questionnaire, questionnaires concerning personal behaviour and substance use, urine drug screening conducted on all participants upon entry into the program and arrest and incarceration information derived from the youth court records six months prior to the intervention implementation. A multiple regression model was utilised to determine the justice system expenditures across intervention groups. The use of a multiple regression model allowed for control of inherent differences between the three treatment groups and within each treatment group, enabled analysis of the different variables that affected the outcome and allowed the researchers to account for participant attrition. Participants were coded on demographic characteristics, home environment, personality dimensions, behavioural factors and treatment contact. The use of statistical procedures, including regression analysis, to control for extraneous variables via matching led to a rating of '3' on the SMS for this evaluation.

Results

Over the course of the 12 month follow-up, data on the impact of treatment on three treatment groups was obtained—ISM, treatment consisting of intensive outpatient counselling with CBT and youths that partook in standard probation or parole. On average, participants in the CBT treatment group produced less than one-third of the expenditure spent on subsequent court referrals and days of detention during investigative period than the regular probation or parole comparison group (\$1,542 per participant versus \$5,034 for the comparison group). The mean criminal justice system expenditure for the ISM treatment group was also slightly higher than the regular probation or parole comparison group (\$5,356 per participant). After controlling for the inherent differences between the groups, the CBT treatment group demonstrated a statistically significant lower justice system expenditure by comparison with the regular probation or parole—no statistically significant difference was demonstrated by the ISM treatment group. It was concluded that the difference in the cost of justice system expenditures resulting from juvenile recidivism between the CBT treatment group and the regular probation or parole comparison group was due to the program's implementation.

Participants in the CBT group were statistically less likely to complete the CBT program than for participants in the ISM and regular probation or parole group. The higher drop-out rate for CBT treatment was attributed to the extensive amount of cognitive skills training and counselling involved within this treatment.

Economic analysis

A CBA was conducted on the CBT intervention program by comparison with regular probation or parole. The cost of treatment in the CBT treatment group was estimated by calculating the additional funding used to finance the additional personnel and resources required to undertake the CBT program. Benefits were determined by estimating the savings from a reduction in court referrals and days of detention, and short-run local justice system expenditures for juvenile participants who undertook the CBT intervention.

The marginal cost of the CBT intervention for each participant in the CBT treatment group, compared with the regular probation or parole intervention,

was estimated at \$1,493 per youth. Conversely, the marginal benefit of the CBT treatment, holding all else constant, was estimated at \$2,928 per youth.

Dividing total benefits by total costs produced a desirable cost-benefit ratio of 1.96:1. For each \$1 that was invested in this program, taxpayers received nearly \$2 in short-term benefits from reduced justice system expenditure.

Robertson and his colleagues (2001) believe the benefits of this program have been conservatively estimated as benefits of the CBT treatment that fall outside short-term justice system expenditure have been excluded in this analysis, including the indirect and long-term costs that a society incurs due to a delinquent's criminal behaviour over their lifespan.

One issue worthy of consideration is the limited information regarding the calculation and monetisation of benefits. Robertson, Grimes and Rogers (2001) state that holding all else constant, relatively lower justice system expenditure can be expected for the CBT treatment group as a result of the fewer referrals and days detained incurred by the CBT treatment group by comparison with the regular probation or parole group. A multiple regression model was constructed to estimate the change in justice system expenditure resulting from the intervention; however, the details regarding the input into this model are not discussed.

Confidence in the outcome of Robertson, Grimes and Rogers' (2001) use of CBT as a community-based intervention technique to reduce recidivism and CBA is relatively high. From the information available, it appears that cognitive behavioural treatments for juvenile delinquent can have a significant influence on the reduction in short-term justice system expenditure through a reduction in referrals and days detained. Further, the CBA had a relatively narrow focus and therefore, the study is likely to have underestimated the program's cost-effectiveness. Of concern is that the details regarding the monetisation of the local justice system expenditure is relatively vague with limited information on the derivation of the figure to represent the reduction in referrals and days detained. Another consideration is the substantially higher drop-out rate for the CBT treatment, compared with the regular probation and parole control group and ISM treatment group, indicating a balance is required between providing the intensive cognitive treatment and maximising program participation. Overall, however, the use of a high quality

experimental-control group design to assess program effects reduced the threats to internal validity. The use of regression modelling to statistically match the comparison group to the treatment groups increased confidence in the program effects and allowed the analysis of different demographic, social and behavioural on justice system expenditure and completion rates. Overall, the economic assessment is accepted, although a number of factors, previously discussed, indicate more work is required in relation to the CBA.

Case study 4

Impact and cost-benefit analysis of the Anchorage Wellness Court

Roman, Chalfin, Reid and Reid 2008

Extensive research linking criminality among a substance-using population prompted the implementation of substance abuse treatment in addition to criminal justice system monitoring. Since 1999, the Anchorage Wellness Court (AWC) in Alaska has been responding to offenders serving misdemeanour driving under the influence (DUI) offences with the goal of reducing alcohol-related offending. Although clinical staff and prosecutors determine the participant's eligibility to undertake the program, involvement is voluntary. Incentives for successfully completing the program include reduced jail terms and fines. The intervention project was implemented over a period of 18 months, with a four year follow-up. The treatment outcomes assessed included therapeutic jurisprudence. Therapeutic jurisprudence is an interdisciplinary approach which

seeks to assess the therapeutic and counter-therapeutic consequences of law and how it is applied and to effect legal change designed to increase the former and diminish the latter. It is a mental health approach to law that uses the tools of the behavioural sciences to assess the law's therapeutic impact, and when consistent with other important values, to reshape law and legal processes in ways that can improve the psychological functioning and emotional well-being of those affected (Winick 2000: 33).

Design and method

This experiment used a quasi-experimental, pre-/post-design by comparing the differences in criminal recidivism between the treatment group (an opt-in group and an opt-out group) and the comparison group. The treatment group involved substance abuse treatment as well as other forms of therapeutic intervention, including moral recognition therapy, recovery meetings, employment and financial coaching, case management and substance abuse monitoring, judicial surveillance and criminal justice assistance.

Of the 277 individuals eligible for the AWC on the basis of eligible DUI misdemeanours, 136 individuals were referred to the treatment program, while the remaining 141 had no involvement with the program and formed the comparison group. Of the 136 individuals referred to the treatment group, 45 did not formally enrol in the treatment program and were categorised as the opt-out group, with the remaining 91 formally enrolling in the program, opt-in group. Of the 91 participants who were referred to the program and completed the treatment, 44 completed the program successfully, while 47 failed the program. The researchers' note that a time lag existed between referral and formal enrolment, sometimes months, creating the likelihood that participants were receiving substance abuse treatment and program services through the AWC program during this time even though they had been included in the opt-out group.

The duration of the treatment covered an 18 month period. Outcomes were observed at follow-up intervals 24, 30, 36 and 48 months.

To examine the effectiveness of the treatment, the comparison group undertook judicial sentencing as usual. To ensure the comparison group was comparable to the treatment group, the researchers obtained arrest and conviction data for all participants in the evaluation from the Alaska Department of Public Safety. A database that contained information on the characteristics of the study's participants and included all those referred to AWC between 2000 and 2004 was maintained by the University of Alaska. The comparison group comprised a sample of matched individuals who were arrested in the same period and were eligible for AWC but were not referred. Participants from the comparison group were matched to AWC treatment participants on

demographic characteristics, current charge and prior arrest history via the use of a propensity score model. The use of a propensity score model allowed for control of inherent differences between the treatment group and the comparison group, and to account for the effect of self-selection into the AWC program, a multivariate model was generated. The use of statistical procedures, including multivariate outcome models, to control for extraneous variables via matching resulted in the rating of '3' on the SMS, determined by comparison with Table 1 Maryland SMS.

Results

Over the course of the 18 month AWC treatment and four year follow-up period, the outcomes of the treatment were complex. Participants in the AWC treatment program fared better than the comparison participants on the basis of new arrest likelihood and the number of new arrests; however, the AWC treatment program was more expensive to administer and the damage from new offending was greater. At the 24 month follow-up, 37 percent of the participants in the treatment group had been re-arrested, compared with 53 percent of the participants in the comparison group. This statistically significant difference continued through to the 48 month observation point, where 47 percent of the treatment group had been re-arrested compared with 66 percent of the comparison group. Participants in the treatment group were also less likely to be convicted, but the difference was only statistically significant at 24 months. Although fewer participants in the treatment group were re-arrested than the comparison group, the treatment participants who reoffended committed crimes that were more serious than average, resulting in a higher cost to the criminal justice system and society than the comparison group.

The results are more positive if the participants within the AWC treatment group who elected to opt-out of the program are excluded from the analysis, leaving the participants who completed the program (the opt-in group). The opt-in group had better outcomes at 24 months, with 26 percent of the opt-in group having been re-arrested, compared with 53 percent of the comparison group. These differences are significant and were still apparent at 48 months where 42 percent of the opt-in group had been re-arrested

compared with 66 percent of the comparison group. Participants in the opt-in group were also less likely to be reconvicted at 24, 30, 36 and 48 months.

Conversely, for the opt-out group, the researchers reported no effect or negative effects in offender recidivism compared with the comparison group. The rate of re-arrest at 24 months was greater for the opt-out group (55%) compared with comparison group (53%) although not significantly so. Although 55 percent of the opt-out group had been re-arrested at 48 months compared with 66 percent of participants in the comparison group, this difference was not statistically significant. The odds of re-arrest for the opt-in group and the opt-out group by 48 months are virtually the same. Moreover, the effect of the opt-out group on criminal justice system and public costs as a result of new offending was significantly higher than the comparison group.

Economic analysis

A CBA was conducted on the AWC treatment program by comparison with standard sentencing. The cost of treatment in the AWC treatment group was provided by individuals associated with the court system and treatment providers. It was estimated by calculating the additional funding used to finance the additional personnel and resources required to undertake the AWC program. Generally, data on the costs of the program were obtained from budgets or expense reports and information on quantities was obtained from administrative data. For each participant, cost was estimated based on hearing cost and treatment cost. Treatment costs were estimated based on the number of days the individual in the AWC treatment group participated in the program.

Benefits quantified in this CBA arise from a reduction in recidivism associated with alcohol use as a consequence of the AWC treatment program. This evaluation quantified benefits to a number of different parties arising from a reduction in the number of re-arrests and reconvictions of offending. These included police agencies, court time, community supervision agencies corrections and private citizens. The costs of harm to victims for individual crimes were based on estimates by Cohen, Miller and Rossman (1993) and Rajkumar and French (1997). The CBA incorporated both tangible and intangible costs of victimisation to individuals.

The cost for each participant in the AWC opt-in treatment group was estimated at \$3,900 per youth. The marginal benefit among the AWC opt-in group, compared with the comparison group, resulted in a saving of about \$13,400 per participant after 24 months. The opt-in treatment group contributed to net benefits (the benefits minus the costs) of \$9,500 after 24 months. Dividing total benefits by total costs produced a desirable cost-benefit ratio of 3.44:1 after 24 months. That is, for each \$1 that was invested in this program, taxpayers received nearly \$3.44 in benefits after 24 months. Differences between participants in the opt-in treatment group and the comparison group were positive and statistically significant at 24 and 30 month follow-up but were not statistically different after 30 months. As a result of the decline in treatment effects, the author estimates the cost-benefit ratio declined to 1.25, resulting in a \$1.25 return on investment to taxpayers for every \$1 invested in the treatment at 40 months.

The entire treatment group, including the opt-in group as well as the participants referred to the AWC program who received limited treatment, contributed to a net cost to society (negative benefit) of more than \$12,241 per participant after 24 months; a dramatic difference to the \$9,539 net benefit the average AWC-treated opt-in participant contributed to society after 24 months. The negative benefit findings for the entire treatment group is driven by the inclusion of the opt-out participants who received limited treatment and had average negative benefits of \$15,900–17,400, counteracting the positive benefits the opt-in treated participants received. The size of the negative benefit the opt-out group incurred was substantial and added an additional cost of about \$700 for treating each member of the opt-out group.

Roman et al. (2008) believe the benefits of this program have been conservatively estimated since the benefits of the AWC treatment that fall outside expenditure relating to criminal behaviour have been excluded in this analysis.

An area of concern is the absence of a final cost-benefit figure for each group. Although extensive information is provided regarding the costing of the

opt-in group, only net benefits are estimated for the opt-out group and comparison group. Due to the complexity of the results, a final ratio to express the cost effectiveness of the program would aid understanding of the program effects. The author has estimated the CBA ratio based on the costs and benefit figures provided in the CBA study.

Another consideration raised by Roman et al. (2008) is the inclusion of new arrests, according to timing, within each group. Re-arrests of participants in the treatment group were only recorded once they were referred to AWC. However, re-arrests of individuals in the comparison group were recorded at the time of their new arrest. Therefore, a number of arrests were counted for the comparison group that were not counted for the treatment group.

It is considered that the CBA of Roman et al. (2008) to measure the effectiveness of the AWC in reducing alcohol-related offending is positive. Although the results regarding the effectiveness of the program are complex, the CBA is comparatively robust. The CBA had a relatively extensive focus on the benefits incurred and included those accruing to potential victims and taxpayers as a result of reducing recidivism. The avoided victimisation costs also incorporated intangible costs, rarely included in CBA. Further, the program effects suggest the study underestimated the program's cost-effectiveness due to the exclusion of effects upon morbidity and mortality. Moreover, details regarding the monetisation of the local justice system expenditure and cost to crime victims are detailed with extensive information on the determination of the figures. Although the researchers note the lack of exact data regarding the amount of treatment each participant received, estimates provided appear reasonable. The use of a high quality experimental-control group design to assess program effects reduced the threats to internal validity and the use of statistically matched comparison group increased confidence in the program effects and allowed the analysis of different variables affecting levels of recidivism for alcohol-related offending. Overall, the economic assessment of the intervention program is accepted.

Case study 5

A detailed cost analysis in a mature drug court setting: A cost-benefit evaluation of the Multnomah county drug court

Carey and Finigan 2004

This study was motivated by the expansion of drug courts in the United States and the lack of comprehensive examination of costs and benefits of these courts. The purpose of drug courts is to provide an alternative to traditional court processing of offenders identified as having a drug addiction, by incorporating drug dependence treatment. This program assessed costs and benefits on an overall and agency-by-agency basis. One hundred and fifty-five randomly selected individuals were intensively tracked for at least 18 months; 87 individuals participated in the Multnomah County drug court and 68 experienced traditional court processing. This information formed the basis of individual cost allocation to a wider sample of 1,167 individuals—594 drug court participant group members and 573 comparison group members. The final sample was reported to contain 1,173 individuals, made up of 594 drug court participants and 573 comparison participants, although the 1,173 figure appears to be an error and should be 1,167.

Design and method

This study used a quasi experimental-control, pre-/post-design by comparing the differences in criminal recidivism between the drug court treatment group and the comparison group. The treatment group involved physical examination, drug and alcohol and mental health assessment and substance abuse treatment sessions. The comparison group involved traditional court processing and was comprised of a sample of matched individuals who were arrested in the same period and were eligible for drug court but did not undertake any drug court treatment session.

Two samples were selected in this study. The first sample consisted of 155 individuals who were followed through the criminal justice process in great detail—the 'intensive sample'. This sample included 87 individuals who participated in the drug court (the treatment group) and 68 who experienced traditional court processing (comparison group).

To ensure the comparison group were comparable to the treatment group, the researchers obtained arrest and court data for all participants in the evaluation from a number of agencies. Participants from the comparison group were matched to drug court treatment participants on demographic characteristics (ethnicity, gender and age), criminal history and previous treatment episodes two years prior to arrest via the use of aggregate matching. The use of aggregate matching allowed for control of inherent differences between the treatment group and the comparison group. The use of statistical aggregate matching to control for extraneous variables has resulted in the rating of '3' on the SMS for the evaluation, determined by comparison with Table 1 Maryland SMS.

The second sample was larger, consisting of 1,167 individuals who had been arrested for drug court eligible crimes between 1999 and 2000. These individuals were selected from a database maintained by the public defender's office and labelled the 'administrative sample'. The administrative sample was split into those who participated in the drug court (594 individuals) and those who received traditional court processing (573 individuals). Information collected on the intensive sample was more in-depth and provided the basis for allocation of expenditure to administrative sample.

The follow-up covered an 18 month period for the 155 participants and those selected earlier in the study were followed for 22 months.

Results

Of the 1,167 individuals in the large administrative sample, the outcome of drug court participants was more positive than comparison group participants. Drug court participants engaged in less subsequent criminal activity (shown by fewer re-arrests, bookings and jail time) than those who experienced traditional court processing.

Economic analysis

A CBA was conducted on the drug court treatment program by comparison with traditional court processing. The estimation of cost associated with treating drug court participants and non-drug court participants was obtained from estimates provided by staff members in the court system, law enforcement, probation, treatment providers, district attorney and public defender. Information on the costs was based on budgets or other financial information from 2000, 2001 and 2002 and adjusted to 2002 dollars. Every time a participant in the drug court or comparison group interacted with the system and used system resources, a transaction occurs. Costs associated with each transaction involved both direct and indirect costs. For each participant, invested cost was estimated based on every transaction in the process that resulted from the drug court eligible arrest. The costs invested in the drug court participant and the non-drug court participant were identified for each agency. Unexpectedly, the investment required to process participants in the drug court (\$5,928) was \$1,442 less than the investment in the mainstream process (\$7,369).

Benefits or savings associated with outcomes were estimated for each individual by multiplying the number of re-arrests, bookings and jail time by the cost for that category. These costs were summed to determine the total criminal justice processing costs for that individual. This process was undertaken for all individuals in the comparison group and the drug court treatment group over 30 month follow-up period. The average savings from avoided costs for the treatment condition were then calculated and compared for the final cost calculation.

The comparison non-drug court participant generated unit outcome costs of \$11,311, substantially more than the drug court participant unit outcome costs of \$8,983, over the 30 month follow-up. Therefore, the total saved on avoided criminal justice costs, excluding victimisation costs, was estimated at \$2,328 per drug court treated participant, compared with the non-drug court participant.

The total costs for a drug court participant, including both the investment costs and the outcome costs, were \$14,910. The total costs associated with a non-drug court participant (investment costs and

outcome costs) were estimated at \$18,681. Therefore, the total savings between drug court participants and non-drug court participants was \$3,770 per participant.

The Multnomah County drug court crime reduction and cost-effectiveness findings appear reliable. From the costing estimates provided, the diversion of drug addicted criminals into drug court as an alternative to traditional court processing had a substantial influence on the reduction in future recidivism and proved to be cost-effective. The cost-effectiveness analysis was comprehensive, including both criminal justice system expenditure and victimisation costs. The use of a high quality experimental-comparison group design with pre-/post-test measures to assess program effects reduced the threats to internal validity. The use of aggregate matching to match the comparison group to treatment group and the substantial second administrative sample increased confidence in the program effects. Overall, the CBA of the diversionary program is accepted.

Case study 6

Cost-effectiveness of Connecticut's in-prison substance abuse treatment

Daley et al. 2004

In response to estimates suggesting 83 percent of inmates serving time in Connecticut state prisons had alcohol and drug problems, four treatment programs of increasing intensity were implemented within the Connecticut corrections environment to address this problem. Daley et al. (2004) evaluated the cost-effectiveness of the four levels, or tiers, of prison-based treatment implemented in Connecticut prisons. The four tiered programs involved increasing intensity of treatment and inmates' allocation was based on assessed need for substance abuse treatment. The total sample of 831 inmates comprised 286 inmates who participated in one of four tiered treatments and 545 inmates who did not attend tier programs. Inmates were followed up for 24 months after release from the index incarceration to determine recidivism.

Design and method

A quasi experimental–control, pre-/post-test design was used to test the effectiveness of the treatment by comparing the differences in criminal recidivism between the four treatment tiers and the comparison group. The treatment tiers involved varying levels of drug treatment. Tier One consisted of six group sessions or four group sessions plus two fellowship meetings. Tier One is not considered ‘formal treatment’, with the majority of inmates attending this program identified as having a high drug treatment score. Tier Two involved group sessions three times a week for 10 weeks. Tier Three included four sessions per week for four months, or a total of 64 sessions. Tier Four involves the most intensive treatment of the four tiers with full-time daily treatment in a separate housing unit for six months. Individuals were classified into one of the four tiers based on the highest tier attended.

Five samples were selected in this study. The first sample consisted of 545 individuals who did not participate in the tiered treatment programs (the comparison group). Of the 286 inmates in this study who participated in prison-based drug treatment, 41 inmates received treatment in Tier One as their highest tier attended, 143 inmates participated in Tier Two, 30 inmates participated in Tier Three and 72 inmates participated in Tier Four as their highest tier attended. The follow-up period was two years and the authors collected outcome data on the rate of re-arrest. Data sources included data from the Connecticut Department of Mental Health and Addiction Services, Connecticut State Police, National Crime Information Center and the Connecticut Department of Correction.

To control for differences between the comparison group participants and tier program participants, a regression model was used to control for ethnicity, age, prior criminal history, drug need, mental health need, overall security risk, grades completed and number of non-tier programs attended during index incarceration. Drug need and mental health need scores, as well as security risk, were obtained from two broad assessments conducted at incarceration intake. The use of regression modelling allowed for control of inherent differences between the treatment group and the comparison group. The use of statistical aggregate matching to control for extraneous variables resulted in the rating of ‘3’ on the SMS for the evaluation, determined by comparison with Table 1 Maryland SMS.

Results

Over the course of the 24 month follow-up, data on the effectiveness of different levels of drug treatment on the rate of re-arrest was examined. Daley et al. (2004) developed a regression model to adjust the rates of re-arrest for differences in participant demographics and criminal history between groups. At 12 months, compared with non-attendees, Tier One program participants had no significant differences on re-arrest, Tier Two program participants demonstrated a non-significant decline in recidivism, Tier Three program participants had a significant decline in recidivism as did Tier Four program participants who had lowest rates of re-arrest.

Although there was a reduction in rate of re-arrest for all tier groups, once the regression model adjusted for differences between the groups, the adjusted probability of re-arrest for Tier One attendees was worse than those who had attended no tier programs. Rates of re-arrest for Tiers Two, Three and Four were lower than non-tier attendees. Adjusted probability of re-arrest within one year was 45.9 percent for non-tier attendees, 49.3 percent for Tier One attendees, 37.4 percent for Tier Two attendees, 27.2 percent for Tier Three attendees and 23.5 percent for Tier Four attendees. The effects of the program appear strongest at 12 months. At 12 months, there was a difference of 23.7 percent between non-tier attendance and Tier Four attendance in rates of re-arrest (45.9% and 22.2% respectively), while at 24 months, the difference in rates of re-arrest between non-tier attendance and Tier Four attendance was much smaller – 12.3 percent points (65.1% and 52.8% respectively).

Economic analysis

A CBA was conducted on the tier programs at each level by comparison with inmates who did not attend one of the four programs. The cost of treatment for each tier level program was estimated by calculating funding for additional personnel and resources required for the substance abuse treatments on top of costs necessary to operate the prisons. Generally, data on the costs of the treatment programs was obtained from prison system budgets, interviews with tier program staff and monthly activity data detailing the numbers surrounding the treatment sessions.

Benefits quantified in the CBA arise from a reduction in incarceration costs associated with re-arrest as a consequence of the treatment programs. This evaluation quantified benefits to the Connecticut Department of Correction arising from the avoided costs of re-incarceration. The average cost of re-incarceration was estimated at \$45,536 per inmate based on the average length of incarceration multiplied by the cost per day.

The average cost per participant for each tier level program depended on whether the program cost was divided by the average program attendee or the completed program attendee. The cost for each participant in the Tier One treatment group was the lowest and estimated at \$189 per inmate—the same for attendees and ‘completers’. The cost for each participant in the Tier Two treatment group was \$672 per attendees and \$1,599 per completed attendee. The cost of an average Tier Three attendee was \$2,667, while the cost per Tier Three completed attendee was \$6,882. The cost for each participant in Tier Four was \$5,699 per attendee and \$15,258 per completed attendee. All costs and benefits used in the CBA were based on all tier program attendees, regardless of the attendees’ completion of the program.

The marginal benefit among the Tier Two treatment group, compared with the comparison group, resulted in a saving of \$37,605 per participant after 12 months. The Tier Three treatment participants had net benefits (the benefits minus the costs) of \$31,233 after 12 months, while the Tier Four participants had net benefits of \$20,098 at 12 months. Dividing total benefits by total costs produced a desirable cost-benefit ratio of 5.74, 3.18 and 1.79 for Tiers Two, Three and Four participants respectively after 24 months. Therefore, for each \$1 invested in the program, taxpayers received \$5.74, \$3.18 or \$1.79 respectively in savings from avoided recidivism after 12 months. As the Tier One program was ineffective in reducing rate of re-arrest, therefore incurring costs but producing non-significant recidivism benefits, the marginal benefits were negative \$5,635 per participant.

The benefits of the overall program have been conservatively estimated, since benefits of the treatment that fall outside expenditure relating to incarceration have been excluded from this analysis (ie criminal justice processing costs and victimisation costs avoided due to reduced criminal offending).

Daley et al. (2004) discuss three limitations of this study. First, the limited background information on the inmates who participated in the analysis affected the capacity to use regression analysis to fully control for the differences between groups, or unmeasured variables. Second, no attempts were made to single out the components of the different tier programs that were successful in reducing recidivism. Third, no information was collected on inmates’ substance abuse treatment programs undertaken post jail release.

Additional considerations include the absence of drop-out rates between the different programs. The difference in cost per attendee and cost per ‘completer’ indicate a drop-out effect. Although these rates have not been included, the CBA is based on all costs and therefore includes participants who did not complete the program. Another consideration is that the CBA has a relatively narrow focus in regards to estimating the benefits from a reduction in recidivism, including only those related to re-incarceration as a result of re-arrest. This is likely to have underestimated the benefits of the program by excluding other costs of re-arrests such as criminal justice costs (court costs, police investigation costs etc) as well as victimisation costs. As the effects of the program are most dramatic after 12 months, there is a need for long-run costs occurring after 12 months to be calculated.

The use of CBA by Daley et al. (2004) to measure the effectiveness of varying levels of substance abuse treatment implemented in Connecticut’s prisons produced convincing results. From the costing analysis conducted, the provision of substance abuse counselling for inmates assessed as having a substance abuse problem had a significant influence upon the reduction in future recidivism and proved to be cost effective in the short term. The effectiveness of these four programs was not uniform however, with Tier One showing no effect or a negative effect upon the future re-arrest rate. More intensive substance abuse treatment, such as that completed in Tiers Two, Three and Four, did produce a positive effect upon likelihood of future re-arrest. Although the CBA had a relatively narrow focus, only including benefits relating to re-incarceration, this is likely to have underestimated the benefits of the program. The use of an

experimental-control group design to assess program effects reduced the threats to internal validity and the use of regression modelling to adjust for differences between the comparison group and the four treatment groups increases confidence in the program effects. Overall, the economic assessment of the program's intervention is accepted.

Case study 7

Age 21 cost-benefit analysis of the title I Chicago Child-Parent Centers

*Reynolds, Temple
Robertson & Mann 2002*

Although the federally funded Chicago Child-Parent Center (CPC) program has been operating since 1967, Reynolds et al. (2002) produced the first CBA conducted on this intervention program. The CPC is located in Chicago Public Schools in the poorest neighbourhoods and provides educational and family support to low-income children aged three to nine years. This intervention program operates on a substantial scale with over 100,000 children participating across 23 sites. The program has been associated with improved school achievement, greater rates of high school completion and with lower rates of remedial education services, juvenile delinquency and child maltreatment. The program involves three intervention points—preschool age, school age and extended intervention. Reynolds et al. (2002) used data from a cohort of 1,539 children born in 1980 who participated in the study as either program participants or comparison group children up to the age of 21 years. The final sample assessed at age 20 included 1,286 youth—841 youths in the treatment group and 445 youths in the comparison group.

Design and method

A quasi experimental-control design was used to test the effectiveness of the treatment by comparing the differences in a range of benefits, including rates of juvenile arrest, between the treatment group and

the comparison group. The treatment involved educational and family-support services to children between the ages of three and nine years. Relative to the comparison group, the assessment involves three treatment categories—preschool participation, school-age participation and extended participation. Participants in the 'preschool' group began the CPC program at ages three or four years and on average completed 1.5 years program participation. Children classified as 'school-age' began the CPC program either in the first or second grade and completed one to three years program participation. Extended intervention involved children who undertook the CPC program from preschool to second or third grade (4 to 6 years program participation). Of the 1,539 children in this study, 989 children participated in the CPC program and a random sample of 550 eligible children did not attend the CPC program, instead enrolling in an all-day kindergarten program. Of these, 14.8 percent attended the Head Start preschool, a program similar to the CPC program. The final sample after 15 year follow-up included 841 children in the intervention group, with a comparison group of 445. The comparison group included low-income children who did not attend the CPC program, instead enrolling in an all-day kindergarten program.

The preschool program runs during the nine month school year for three hours per day, five days per week and also includes a six week summer program. The preschool participation involves a multitude of services, falling under the categories of outreach services, parent component, curriculum component and health services. The school-age intervention is available to any child in the school providing the CPC program, either in first and second grade or first through third grade in other sites. The school-aged participation involves a parent component, curriculum component and school-wide services. Intensive parental participation is required and includes parents volunteering in the classroom, attending school events and field trips and completing high school.

The program's intervention evaluation covered children born in 1980 who attended CPCs in preschool and kindergarten from 1983–86. The follow-up period was 15 years and the authors collected outcome data on the expenditures relating to school remedial services of grade retention and

special education, criminal justice system expenditures, child welfare system expenditures, avoided tangible costs to crime victims and projected adult earnings and tax revenues.

To examine the effectiveness of the intervention program, a comparison group was established of persons who did not participate in the program. Characteristics of participants were measured at the start of the study, from data collected from family surveys, school records and justice system records up to current age of 21 years. Although program selection was not random, the participants in the treatment group and the comparison group were found to be similar on many child and family characteristics. A family-risk index provided a summary of these characteristics, with the means equivalent between the two groups. The family-risk index was made up of ratings on parental education, unemployment status, single-parent family status, eligibility for subsidised lunches, neighbourhood poverty and four or more children in family. The treatment group was more likely to live in neighbourhoods with higher poverty and higher unemployment than the comparison group. A set of covariates were included in the model to account for measured differences between groups including the family-risk index and different program sites; however, no attempt to adjust for selection bias occurred in the regression analysis. The use of matching to control for extraneous variables resulted in the rating of '3' on the SMS for this evaluation, determined by comparison with Table 1 Maryland SMS.

Results

Positive outcomes were shown in the 15 year follow-up period relating to school achievement, high school completion, remedial education services, juvenile delinquency and child maltreatment. Preschool participants, compared with the control group, had a higher rate of high school completion (49.7% versus 38.5%), a lower rate of juvenile arrest (16.9% versus 25.2%), a reduction in special education placement (14.4% versus 24.6%) and a reduction in the rate of the individual repeating a grade by age 15 (23.0% versus 38.4%). Preschool participants were also 51 percent less likely to have court-reported child maltreatment by age 17 years (5%) than participants in the control group (10.3%).

School-aged intervention was associated with fewer program outcomes than preschool participants. By comparison with the control group, the school-aged intervention group experienced greater school achievement at ages nine and 15 years, reduced need for school remedial services, special education placement and were less likely to repeat grades compared with the control groups. No statistically significant differences existed for other factors examined. Relative to program participation for one to four years, program participation in extended intervention (4 to 6 years) was associated with the following outcomes—statistically significant higher levels of school achievement and school completion, decreased need for school remedial services and lower rates of child maltreatment.

Effects upon criminal activity were also apparent. Preschool participants at the age of 18 years had a significantly lower rate of juvenile arrest (17%) compared with the comparison group (25%), demonstrating a 33 percent reduction in the juvenile arrest rate. At the age of 18 years, preschool participants were also significantly less likely to be arrested for violent offences (9%) compared with the control group (15%). For school-aged participants, no group differences were statistically significant for juvenile arrest, high school completion, highest grade completed, or child maltreatment. Being an extended program participant was marginally associated with a lower rate of arrest for violent offenses, although it was not statistically significant ($p=0.099$).

Economic analysis

A CBA was conducted on the three program categories in relation to treatment exposure and age of intervention by comparison with children who did not attend the CPC program. The costs and benefits are estimated in 1998 dollars, discounted at three percent. The cost of the CPC program included taxpayer costs (including staff costs), family and community support, administration, operations and maintenance, instructional materials, transportation and community services, school-wide services, school district support, and capital depreciation and interest. Costs for the preschool program were based on the operational budget for the 1985–86 year and invested costs for the school-age program are based on expenditures for the 1986–87 year.

Benefits quantified in this CBA arose from reductions in educational expenditures for grade retention and special education, reductions in criminal justice system expenditures, avoided tangible costs to crime victims, reductions in child welfare system expenditures and increases in adult earnings and tax revenues from increases in educational attainment. The longitudinal study included the program's measured benefits through to age 21 years and projected benefits beyond age 21. Three types of benefits were identified:

- benefits to participants that accrued to the child and parent attending the program, but did not directly affect others in society;
- benefits to the general public including avoided costs that the public received as a result of program intervention and;
- benefits to society as an aggregate of the benefits to the participants and benefits to the general public.

The average cost per child for 1.5 years of preschool participation was estimated at \$6,692. The average cost per child for two years of school-aged participation was estimated at \$2,981 and the additional cost of extended program participation was \$4,068 per participant (above and beyond less extensive program participation). The benefits of the program were estimated as \$47,759, \$4,944 and \$24,772 for the preschool program, the school-age program and the extended program respectively (on a per participant basis).

Relative to the comparison group, the net benefit (the benefits minus the costs) per preschool participant was \$41,067 at the 15 year follow-up. The school-age treatment participant contributed to net benefits of \$1,963, while the net benefits per extended program participant amounted to \$20,715 at the 15 year follow-up. Dividing total benefits by total costs produced a desirable cost-benefit ratio of 7.14, 1.66 and 6.11 for preschool program, school-age program and extended program respectively. Therefore, for each \$1 that was invested in this program, taxpayers received \$7.14, \$1.66 and \$6.11 in savings after 15 years.

As the Chicago Longitudinal study spanned 15 years, some of the program's benefits were clearly likely to occur years after the program's invested costs had been incurred. The future costs and benefits of the program were converted to 1998 dollars to adjust

for inflation and using a discount rate of three percent to convert future costs and benefits to their present value. The selection of the three percent discount rate was based on the US Public Health Service and the US General Accounting Office agreed rate. As the program's benefits change as a result of varying the discount rate, the inclusion of a sensitivity analysis to analyse the effects of different discount rates strengthens the CBA. Program benefits have been estimated using discount rates from zero percent to seven percent. Even at the higher discount rates (5% to 7%), economic benefits of preschool participation exceeded program costs. Economic benefits up to the discount rate of seven percent also exceeded program costs for extended participation. The benefits of school-aged participation were more influenced by alternative discount rates, although total benefits exceeded school-age cost at a seven percent discount rate.

One limitation of the study, discussed by Reynolds et al. (2002) is the reliance on projected rather than measured effects for increased earning capacity over the participant's lifetime and savings in adult criminal justice system costs and avoided victimisation costs. Although these costs are difficult to predict with certainty, they are based on the educational attainment and juvenile crime behaviour at the start of adulthood, which is described as the strongest predictor of later economic position and criminal activity. For preschool participants, increased earning capacity projected from higher educational attainment accounted for the largest benefit of program benefits; accounting for \$20,517 of the total \$47,759 estimated benefits. For school-aged program participants, the projected earnings/compensation was \$732 out of \$4,944 total benefits. Extended program participations projected earning capacity increased by \$8,610 of the total increased benefits of \$24,772.

The benefits of this program have been conservatively estimated, as benefits that fall outside non-labour market benefits have been excluded from this analysis. This is likely to have led to an underestimation the benefits of the program. This study also excludes benefits such as health status, welfare usage, fertility choices and the amount of schooling obtained by participant's children. Furthermore, the program effects are conservative because a number of children in the comparison group participated in

other interventional programs in kindergarten that provided educational enrichment, unlike the typical experimental design where the control group receives no intervention. The attrition rate was also relatively low at around 15 percent.

The CBA used by Reynolds et al. (2002) to measure the effectiveness of the CPC produces convincing results. From the costing analysis conducted, the provision of comprehensive and intensive services to children and families over a number of years can translate into substantial savings for both the program participants and the taxpayer. The outcomes of the different categories of intervention were not uniform, with preschool intervention showing the greatest economic benefit for the cost of the program. Extended intervention provided the next most positive economic return with school-aged participation just breaking even. Results demonstrate that earlier intervention produces greater returns than later intervention. The CBA had a comprehensive focus, including benefits relating to a number of aspects of the participant's life influenced by the intervention program. The calculation of benefits and costs were rigorous and provided information regarding the basis for estimates. The use of a quasi-experimental design to assess program effects reduced the threats to internal validity and the use of matching to adjust for differences between the comparison group and the intervention groups increased confidence in the program effects. Overall, economic assessment of the program's intervention is accepted.

Case study 8

Evaluation of two intensive regimes for young offenders

Farrington et al. 2002

This study evaluated two intensive treatment programs designed to address juvenile offending, implemented in the United Kingdom. Both programs incorporate military themes such as drilling, physical training, Outward Bound courses and challenging activities. The Thorn Cross High Intensity Training (HIT) Centre incorporated rehabilitative treatment in addition to military elements; while the Colchester Military Corrective Training Centre (MCTC) used an

exclusively military structure. The HIT analysis used data from a cohort of 184 experimental and 130 comparison young offenders. The HIT Centre opened in mid 1996 and was still operating in 2002; the MCTC for civilian young offenders program opened in 1997 and ceased operating in 1998. Farrington et al. (2002) conducted extensive CBAs for both programs, although only the Thorn Cross HIT Centre has been analysed here. It is worth noting the CBA of the MCTC program yielded an unfavourable benefit-cost ratio, meaning that program costs outweighed program benefits.

Design and method

This experiment used a quasi experimental-comparison, pre-/post-test design that compared the differences in criminal justice system expenditure between the experimental treatment group and the comparison group. Youth offenders were considered eligible for undertaking the HIT regime if they were males aged 18–21 years, had approximately six months to serve, were suitable for open conditions (therefore excluding sex offenders and serious drug dealers), had an IQ of 80 or more and did not have a history of mental illness. Issues arose around obtaining the treatment sample due to the delay in identifying and assessing youth offenders for the program and the mutual exclusivity of two of the criteria. Offenders with six months left to serve would have received a sentence of at least 18 months, which is an unlikely sentence for an 18–21 year old unless he was a serious violent offender or recidivist offender, therefore making him unsuitable for open conditions. The sample was increased by relaxing the classification for open conditions if the youth offender was considered to be a low risk to the public. The treatment group involved five phases including initial assessment, basic skills, vocational training, pre-release training and community placement.

Delinquent offenders who were not interested in participating in the HIT Centre, or who threatened to abscond, were not included in the treatment cohort. The comparison group consisted of participants who were eligible according to the specified criteria but not selected to partake in HIT program. The primary reason they were not selected was because they had less than six months of their sentence left to

serve. If they were perceived to lack motivation for the HIT program or their current institution suggested they were not suitable, they were also excluded from the treatment group.

A total of 314 participants initially entered the study. The comparison group included 130 juvenile offenders and the HIT treatment group included 184 juvenile offenders. In the follow-up period, data on 11 youth offenders (8 treatment and 3 comparison youth offenders) could not be located in the Offenders Index or the Police National Computer (PNC). Therefore, the one year analysis was based on 303 youth offenders (176 treatment and 127 comparison youth offenders). An additional treatment individual could not be located at year two follow-up, resulting in the two year analysis being conducted on 302 youth offenders (175 treatment and 127 comparison youth offenders). The completion rate of the HIT youth offenders was 57.6 percent. Of the 184 offenders who started the HIT program, 43 did not complete Phase 1–4 and another 35 did not complete Phase 5, leaving 106 treatment ‘completers’.

The duration of the treatment program covered 25 weeks. The follow-up period was one and two years after release.

The comparison group undertook their remaining sentence in youth offender institutions. To ensure that the comparison group was appropriate, the researchers undertook matching of youth offenders in the comparison group to HIT youth offenders on the basis of the Copas, Ditchfield and Marshall (1994) risk of reconviction score. Participants were coded on risk of reconviction score, which measures variables including age at this sentence to imprisonment, length of sentence, principal offence type for which serving this sentence, age at first conviction, number of prior convictions and number of prior imprisonments.

Youth offenders in the comparison group were identified for approximately two-thirds of treatment offenders. All of the potential treatment offenders were interviewed, undertook psychological tests and had their records checked before admittance into the program. The use of statistical procedures, via matching, for comparison to extraneous variables resulted in the rating of ‘3’ on the SMS, determined by comparison with Table 1 Maryland SMS.

Results

Over the course of the 24 month follow-up, data on the effectiveness of the HIT program on the rate of reconvictions was examined. Predicted and actual reconvictions were examined for both treatment and comparison young offenders at one and two years after release. Predicted reconviction scores were based on the Offenders Index, while actual reconvictions were obtained from the PNC.

Each youth offender, in both the comparison and treatment group, had two reconviction prediction scores calculated. The first score calculated the expected probability of reconviction at one year after release and the second at two years after release. The scores were estimated from data collected on the Offenders Index relating to age at sentence, age at first conviction, number of previous convictions, number of previous imprisonments and number of previous offences of various types.

The participants in the HIT treatment group (176 in total) were significantly less likely than predicted to be reconvicted in year one (predicted 47%, actual 35%), while comparison youth offenders, at year one, were reconvicted at a rate similar to predicted (predicted 56%, actual 55%); after controlling for inherent differences between treatment and comparison offenders. Conversely, at two year follow-up, both HIT offenders (predicted 75%, actual 76%) and comparison group youth offenders were reoffending as predicted (predicted 66%, actual 65%); that is, no treatment effect.

Analysis of reconviction rates also examined 105 offenders who successfully completed the HIT program and matched comparison successes. At one year treatment, successes were less likely to be reconvicted than predicted (predicted 41.5%, actual 31.4%), while comparison successes were likely to be reconvicted similar to rate predicted (predicted 50.5%, actual 49.4%). This small effect disappears by two years as treatment and comparison group successes were reconvicted similar to predicted rate, providing a negligible benefit of the program.

Analysis of treatment effects among ‘non-completers’ found that treatment ‘non-completers’ were less likely to be reconvicted in one year than predicted (predicted 55%, actual 39%), while comparison non-completers were likely to be reconvicted as often as predicted (predicted 67%, actual 66%). At two years, treatment ‘non-

completers' were still less likely than predicted to be reconvicted (predicted 74%, actual 69%), while comparison 'non-completers' were more likely than predicted to be reconvicted (predicted 83%, actual 86%). At one year follow-up, the HIT program was found to be more effective for medium and high-risk youth offenders, those sentenced for non-violent offences and more experienced offenders. Although, the HIT regime was found to only delay post release reoffending by approximately two months.

Economic analysis

A CBA was conducted on youth offenders who undertook the HIT by comparison with youth offenders who participated in a standard youth offender institution. The costs and benefits were estimated in 1999 dollars. The cost of the HIT program was based on the Prison Service Annual Report 1997–98. The cost per offender for an average 25 week stay was estimated at £10,929 for the HIT program and £8,488 at the standard youth offending institution. Therefore, the HIT treatment program cost an additional £2,441 compared with the standard regime (per inmate per program).

Benefits quantified in this CBA were based on estimates of the costs of different crime types in 1999 for the Home Office (Brand & Price 2000). These estimates incorporate security expenditure, value of property stolen and damaged, emotional and physical impact on victims, lost output, health services costs and criminal justice system expenditures. The estimates for non-criminal justice costs and of criminal justice non-disposal costs were updated to Home Office estimates in 2001.

As the treatment youth offenders had an inherent lower risk of reconviction than the comparison individuals, the average costs of crime were adjusted to match the distribution of prediction scores of the comparison youth offenders. The adjusted average treatment offender cost society £7,423 in offences committed after release from HIT program, while the average comparison offender resulted in costs to society of £9,903 as a result of reconvictions. The benefits of the program were therefore estimated as £2,480 as a result of the treatment offender committing fewer offences.

Relative to the comparison group, the net benefit (the benefits minus the costs) per treatment

participant was £39 after two year follow-up (£2,480–2,441). Dividing total benefits by total costs produced a marginal desirable cost-benefit ratio of £1.02. Therefore, for each £1 that was invested in this program, taxpayers received £1.02 in savings after two years. Farrington et al. (2002: 27) argue that 'at least five indictable offences are committed for every one leading to conviction' resulting in reconvictions only representing a portion of the offences actually committed. Using this rationale, the benefits for HIT treatment offenders are recalculated upwards by a factor of five, resulting in total benefit for the average HIT youth offender of £12,400 (£2,480 x 5) compared with comparison youth offenders. Or conversely, the HIT youth offender cost society £12,400 less than the comparison youth offender in conviction costs. Recalculating the cost-benefit ratio with the scaled up benefits produced a more desirable cost-benefit ratio of 5.08 (£12,400/£2,441). For each £1 that was invested in this program, taxpayers received £5.08 in savings after two years.

The benefits of this program have been conservatively estimated since benefits of the HIT treatment that fall outside avoided criminal costs (ie health status, welfare usage etc) and benefits beyond two years have been excluded in this analysis. This is likely to have underestimated the benefits of the program, although the benefits surrounding the costs of crime avoided are relatively comprehensive.

One limitation of this study is the comparability of the comparison group to the treatment group. In addition to only two-thirds of youth offenders in the treatment group having matched offenders in the comparison group, the youth offenders in the comparison group had a higher probability of reconviction at the program outset. Although reconviction costs were adjusted to recognise this difference, other differences between the groups (other than risk of reconviction score) were not discussed.

Another consideration is the adjustment to program benefits of the HIT treatment participants to account for the offences committed but not convicted. The researchers argued that for every offence leading to a conviction, five other offences were likely to have been committed. This, too, is conservative as other researchers have indicated that for one in eight burglaries, one in 16 vehicle thefts and over six

categories of indictable crimes, only about one in 30 arrests leads to a conviction (West & Farrington 1977). Although this appears reasonable, the rationale behind settling on the factor of five is unclear.

An additional consideration is the higher rate for 'non-completers' among the treatment youth offenders. Of the 184 individuals in the treatment group, 106 participants successfully completed the program (58%), leaving 70 'non-completers' (42%). Of the 127 comparison participants in standard youth offender institutions, 83 participants successfully completed their program (60%), leaving 44 'non-completers' (34%).

The CBA used by Farrington et al. (2002) to measure the effectiveness of the Thorn Cross HIT program produced marginal, though debatable, results. From the costing analysis conducted, the provision of rehabilitative treatment in addition to military elements translated into savings for both the program participants and the public. In the short term, the treatment program had a positive effect upon reconviction rates of youth offenders after release compared with standard youth offender institutions, with effects fading beyond two years. Although the CBA had a moderate focus (only including benefits related to a reduction in reconvictions), the calculation of benefits and costs were rigorous and provided information regarding the basis for estimates. The factoring up of benefits to account for unconvicted offences committed by the youth offenders post-release seem reasonable, although the rationale behind the basis of five is unclear. Conversely, the factoring up could be an attempt to justify the program. This factoring transforms the CBA from a very ordinary 1.02:1 to a substantially more positive 5:1 ratio. The use of a quasi-experimental design to assess program effects reduced the threats to internal validity and the use of matching to adjust for differences between the comparison group and the intervention groups increased confidence in the program effects; however, it must be noted that questions can be raised regarding the comparability of the comparison group to the treatment group. Overall, the economic assessment is accepted, although a number of factors, previously discussed, indicate more work is required in relation to the CBA.

Case study 9

Benefit and cost analysis of the drug court program

Acumen Alliance

Due to the success of drug courts in United States, the United Kingdom, Canada and Ireland, they have been introduced into Australia. Drug courts provide an alternative criminal justice response to incarceration for offenders whose criminal behaviour is related to drug use/dependency. Taking into account the experiences of other drug courts, the Victorian Drug Court initiative was developed as a response to the failure of custodial sanctions to adequately address drug use and related offending. The first Victorian Drug Court, located in Dandenong, commenced in May 2002 with the objective of protecting the community by focusing on offender alcohol/drug rehabilitation in order to stabilise participants' chaotic lifestyles and reintegrate them into the community. There are two components to the Drug Treatment Order (DTO) issued by the Court:

- treatment and supervision; and
- custodial sentence—of less than two years (suspended to allow for treatment).

The Victorian Magistrates Court determines if individuals are eligible for a DTO based on a list of eligibility criteria. Potential candidates are ordered to undergo an assessment and a report is provided to the drug court as part of the referral process. The drug court Magistrate is then responsible for the supervision of offenders placed on the drug court program under a DTO. A multi-disciplinary team consisting of a case manager, clinician, specialist community corrections officers and a dedicated police prosecutor and defence lawyer will assist the drug court Magistrate in managing and supervising participants on the DTO. If participants breach the DTO, the DTO can be cancelled and participants sentenced to serve the unexpired portion of their sentence.

The drug court program involves three phases and aims to reduce drug/alcohol usage and reoffending. Housing assistance is also provided for those experiencing homelessness or housing problems. A three year evaluation was undertaken, which involved

143 drug court participants and a comparison group of offenders who received standard sentencing and incarceration. Acumen Alliance was funded to assess the cost effectiveness of the pilot program and produced a cost-benefit estimate.

Design and method

This study used a quasi experimental-control, pre-/post-design by comparing the differences in criminal recidivism, unemployment, housing and health and wellbeing between the drug court treatment group and the comparison group; as well as a number of qualitative benefits. The treatment group involved three phases—Phase One involved stabilising health and wellbeing, reduce drug use and cessation of offending (6–9 months), Phase Two involved improving lifestyle changes and health and job prospects (up to 6 months) and Phase Three involved re-integration into work, family and community settings (up to 12 months). The comparison group involved traditional court processing and incarceration for individuals who were arrested in the same period and were eligible for drug court but did not undertake any drug court treatment session.

Health Outcomes International Pty Ltd has compiled four reports to assess the effectiveness of the Victorian Drug Court and this information provides the basis for the Acumen Alliance CBA. The *Cost-Effectiveness Study—Victorian Drug Court*, volume 4 in the series (King & Hales 2004), provides information about the experimental design for assessing recidivism outcomes, while volume 3 outlines the health and wellbeing outcomes.

Two samples were selected for this study. The treatment group consisted of 91 individuals who participated in the drug court and the comparison group included 89 individuals who were randomly selected during the same period and who were on similar charges. The period under review for recidivism was for individuals who participated in the program or comparison group who were incarcerated between May 2002 and May 2004. DTO participants were interviewed at the start of their involvement in the drug court program (baseline), then at three months and six months post-baseline interview. Demographic information

and offending history collected on these participants was provided by the Department of Justice. The target for this drug court was to process 50 DTO participants per year. These individuals were identified by the Magistrates Court as being eligible for drug court based on a number of criteria and were tracked throughout the study.

To examine the cost effectiveness of a drug court, a comparison group was established who experienced traditional court processing and incarceration. The treatment and comparison groups were considered well-matched in terms of age, gender, prior convictions and custodial sentences received; however, information on differences in drug and alcohol dependency and previous treatment episodes between the treatment group and the comparison group was unable to be obtained. The use of rudimentary matching allowed a degree of control of inherent differences between the two groups. Although the groups appear similar, statistical analysis was only conducted on the number of prior convictions between the two groups. The comparison of equivalent comparison group with the treatment group resulted in a rating of '3' on the SMS for this evaluation, determined by comparison with Table 1.

The duration of the program was two years with a follow-up varying from 19 months for evaluating rates of offending and 30 months for other outcomes.

Results

Over the course of the 24 month follow-up, data on the effectiveness of DTO on the rate of reoffending, income support and welfare was examined. The results of the DTO program were determined by Turning Point Drug and Alcohol Centre and Health Outcomes International (Alberti et al. 2004). These figures were utilised by Acumen Alliance to produce the CBA.

The rate of full-time employment for DTO participants more than doubled after the treatment program's implementation (11% before treatment versus 25% after treatment), with the proportion of unemployed DTO participants declining from 86 percent before program's implementation to 54 percent after treatment.

The primary objective was to reduce reoffending rates and this was achieved. Although both the treatment and the comparison groups committed offences at a similar rate for the year observed (on average 3.79 and 3.75 respectively per participant), in the time spent out of custody participants in the treatment group committed offences at a significantly lower rate than the comparison group. This is explained by comparison individuals spending more than twice the amount of time in custody than DTO participants. DTO participants were 23 percent less likely to reoffend than incarcerated comparison individuals once the number of 'free' days was factored in (5.80 offences per 365 free days for the comparison group versus 4.49 per 365 free days for the drug court participants). The rate of offending for DTO participants was also significantly lower in the review period than in the period before treatment (3.79 offences versus 4.90). The rate of reoffending for DTO participants who completed the treatment program was 68 percent less for graduates than for incarcerated comparison individuals (1.84 offences for DTO graduated participants per 365 free days versus 5.80 offences per 365 free days for the comparison group).

Economic analysis

A CBA was conducted by Acumen Alliance on offenders who undertook drug treatment orders by comparison with offenders who participated in standard court sentencing (incarceration). The cost of the DTO program was based on information provided by the Department of Justice. The average ongoing annual investment for the drug court was estimated at \$2.87m per annum, shared by the Department of Justice (\$2.14m) and the Department of Human Services (\$0.73m). The weekly cost per treatment participant was estimated at \$1,103 for the DTO program (\$822 for the Department of Justice and \$281 for the Department of Human Services) or \$57,356 annually. The weekly cost of the comparison incarcerated offender was estimated at \$1,154 (annual cost of imprisonment per prisoner was estimated at more than \$60,000, divided by 52 weeks to obtain the weekly figure). Therefore, the DTO treatment program had a lower cost compared with the standard process. Although the program's duration was two years, the CBA was conducted on an annual basis, annual costs divided by annual benefits.

Benefits quantified in this CBA are based on outcomes provided by Turning Point Drug and Alcohol Centre and Health Outcomes International Reports, Queensland Drug Court Evaluation report, DHS Office of Housing Reports and survey of Drug Court team member. Acumen Alliance allocated monetary figures to the outcomes, classifying them as financial benefits, economic benefits and social benefits. These monetary estimates come from a variety of sources, with the majority based on the *2004 Report of Government Services* (Productivity Commission 2004).

Financial benefits include reduced recidivism, fewer prison days required, fewer services for victims of crime, future drug treatment places, fewer emergency accommodation placements, fewer public housing placements, improved health status resulting in a reduced demand for health services and reduced likelihood of contracting blood borne diseases. Economic benefits relate to a reduced number of crimes, reduced unemployment, improved safety and security in the community and reduced loss of life and disability.

Financial benefits are estimated as \$4,756,678 and economic benefits are estimated as \$11,898,886. Social benefits were still to be valued. This resulted in total benefits for the Victorian Drug Treatment program being valued at \$16,655,564 annually. Therefore, the average benefits generated per participant is estimated at \$333,111 annually (\$16,655,564 divided by 50 participants).

Relative to the comparison group, the annual net benefit (the benefits minus the costs) per treatment participant was \$275,755 after one year of program implementation and one year follow-up (\$333,111–\$57,356). Dividing total benefits by total costs produced a desirable cost-benefit ratio of 5.81. Therefore, for each \$1 that was invested in this program, taxpayers received \$5.81 in savings after one year.

The benefits of this program have been conservatively estimated since social benefits of the DTO treatment that fall outside economic and financial costs (ie reduced costs of preventing crimes, reduced security and insurance expenses, improved quality of life for participants, families and communities, second generation effects, drug free births etc) and benefits beyond one year have been excluded in this analysis.

One limitation of this study is the comparability of the control group to the treatment group. Limited information is provided on differences between the two stating only the averages between the two groups on age, gender, prior convictions and custodial sentences received, with no mention of statistical matching between the two groups in the report by Turning Point Drug and Alcohol Centre and Health Outcomes International. The comparison group was more likely to have a higher number of average offences per individual (50.1) compared with the treatment participant (43.6). Whether this is statistically significant is not discussed. The researchers were unable to obtain information on the comparison group's substance abuse history making it difficult to determine if statistical differences existed between the two groups in relation to drug and alcohol abuse. Another consideration is the small sample size and control group size and the relatively short follow-up period.

Acumen Alliance (2005) estimated the average cost of a criminal act by dividing the total cost of crime for Victoria in 2003–04 (\$1.913b), by the number of crimes recorded in that same year (n=394,822) resulting in a cost of \$4,845 per crime. Acumen Alliance argued crimes committed by drug offenders are at the more serious end of crimes types, therefore, multiplying the average figure of \$4,845 by a factor of 10 to result in \$48,450 being allocated to every criminal act avoided. The basis for this weighting is not stated and to consider every criminal act avoided being a serious crime seems problematic. As the crime reduction estimate accounts for a substantial proportion (\$11,143,500) of the total (\$16,655,564) benefits monetised, a more specific costing of crime types would be beneficial. An improvement would be to determine the specific

crimes avoided, determined by the comparison group, and multiply these by individual values for each crime type, resulting in a more accurate figure of crimes avoided.

Overall, Acumen Alliance's (2005) CBA to measure the effectiveness of the Victorian Drug Court produces positive results. From the costing analysis conducted, the provision of substance abuse treatment and housing assistance translated into savings for both the program participants and the taxpayer. The treatment program had a positive effect upon reconviction rates of offenders after treatment compared with comparison incarcerated offenders. The CBA had an extensive focus, including benefits relating to recidivism, housing situation, employment, health benefits and a lessening of demand for future drug treatment places. The calculation of benefits and costs are rigorous and provide information regarding the basis for estimates. However, the factoring up of benefits to account for seriousness of offences avoided appears too general and the rationale behind the basis of 10 is unclear. Due to a significant portion of benefits resulting from a decline in recidivism, greater detail and categorisation of crimes types avoided is required to ensure accuracy of this figure. Further, although the use of a quasi-experimental design to assess program effects reduced the threats to internal validity, the lack of matching to adjust for differences between the comparison group and the intervention group decreased overall confidence in the program effects, as questions can be raised regarding the comparability of the control group to the treatment group. Overall, there are reservations about accepting the economic assessment due to a number of factors, previously discussed, which indicate more work is required on the CBA.



Other Australian and New Zealand case studies

A number of Australian studies were considered but not included in this report for a range of reasons. Some of these cases are discussed below.

Lind et al. (2002) assessed the cost-effectiveness of processing offenders through the NSW Drug Court as an alternative to conventional sanctions in reducing drug-related crime. This program assessed costs and outcomes on an overall and agency-by-agency basis. This study is the first evaluation of a criminal justice program in Australia to use a randomised control design and one of *the* first, if not the first, to perform a comprehensive CEA of a criminal justice program. However, due to the absence of benefits monetised, this program could not be converted into a CBA and assessed in this systematic analysis.

DataDot Technology (2007) conducted a CBA, reviewed by Pricewaterhouse Coopers, to assess Whole-of-Vehicle-Marking (WOVM) implemented in an effort to address motor vehicle theft in New Zealand. WOVM involves the marking of all major parts of the car by microdots, with around 10,000 microdots attached to the car and each dot containing the Vehicle Identification Number. This has proven successful in deterring organised vehicle theft involving the re-sale of stolen vehicles and parts. For the policy to be implemented, Cabinet approval of a CBA was required in March 2007. This CBA was

excluded from the systematic analysis due to the absence of a control group and the economic assessment being conducted prior to program implementation. The New Zealand Government has adopted a policy of mandatory WOVM.

The Crime Research Centre (2007) at the University of Western Australia provided a comprehensive costing of the invested costs in the WA Diversion Program and discussed the benefits resulting from program implementation. These benefits include savings to society in the form of reduced criminal justice system costs as a result of reduced criminal offending; however, due to the assessment not monetising the identified benefits, the study has been excluded from the systematic analysis.

MM Starrs Pty Ltd (2002) and the National Motor Vehicle Theft Reduction Council (NMVTRC) assessed the effectiveness of compulsory installation of engine immobilisers as a defence against motor vehicle theft in Western Australia. A substantial portion of vehicles stolen were taken by opportunistic thieves for the purpose of committing other crimes and these thieves predominantly targeted vehicles older than 10 years. The NMVTRC was formed to reduce the rate of vehicle theft and was focused on the installation of motor vehicle engine immobilisers. Western Australia has implemented a compulsory scheme since 1999, with the other states and

territories following in 2000. New vehicles and those subject to transfer of ownership were required to have immobilisers fitted for registration to occur. In Western Australia in 2002, 70 percent of vehicles had an immobiliser fitted. The assessment found car theft declined by 17 percent per annum during the scheme; of this decline, 45 percent was attributed to the compulsory immobiliser scheme. Benefits exceeded costs, producing a positive cost-benefit ratio of 1.3. The effectiveness of the scheme was determined by correlational evidence based on a pre-/post-test design. Due to the absence of a comparison group and the reliance on correlational evidence, it is excluded from the systematic review.

Donato, Shanahan and Higgins (1999) discuss the costs and benefits of implementing sex offender treatment programs (SOTP) in correctional facilities for male sex offenders who offend against juveniles. The treatment program consisted of CBT with relapse prevention. The cost of juvenile sex offender treatment programs was based on the average figure estimated from a range of projects in Australia and New Zealand, rather than an individual program. The benefits were calculated based on the costs avoided from a reduction in recidivism. This analysis indicated that if a 14 percentage point reduction in recidivism occurred as a result of SOTP, savings of up to \$39,870 per treated prisoner could eventuate, indicating the treatment could be cost-beneficial. This assessment was excluded from the systematic analysis as it was representative of SOTPs rather than an assessment of an individual study.

Although no CBA has currently been conducted on the Triple P (Positive Parenting Program) program, Mihalopoulos et al. (2007) has completed a threshold analysis to determine the point at which the treatment program will be cost-effective. The Triple P program was developed in Australia (although it is now implemented internationally) to reduce the occurrence of behavioural, emotional or developmental problems in children and adolescents through the provision of parental and family support. The program includes five levels of intervention of increasing involvement,

with substantial research evaluating the program's effectiveness in reducing the prevalence of conduct disorders. The Triple P program threshold analysis estimated the reduction in conduct disorders required for the treatment to break even. Due to the substantial costs that children with conduct disorders impose on society such as a higher demand for health services, educational and criminal justice system costs, a reduction of only 1.5 percent in the prevalence of conduct disorders was conducted for the program to be cost neutral. Mihalopoulos et al. (2007) have estimated a reduction in prevalence of conduct disorders resulting from Triple P treatment ranging from 26 percent to 48 percent. This assessment is a conservative estimate of the program's benefits as only benefits relating to averted conduct disorders up to the age of 28 years have been included in this assessment, ignoring lifetime costs and other substantial benefits. As no comprehensive quantification of the benefits has been conducted nor a CBA, this program has not been reviewed as part of this systematic analysis.

Access Economics (2006) prepared a CBA to determine the economic value in subsidising Opal Unleaded Petrol in central Australia as a means to combat incidences of petrol sniffing in Aboriginal communities. The inclusion of the value of a healthy life in the benefits had a substantial effect on the annual net benefits. The inclusion of healthy life benefits results in a net gain of \$4.3m in the worst case, \$27.1m in the base case and \$53.7m per annum in the best case. However, if healthy life benefits are excluded, the worst and base case produces a loss of \$14.8m and \$1.5m respectively but a net gain of \$12.7m in the best case. The inclusion of benefits gained from reducing morbidity and premature death results in the intervention being assessed as cost-beneficial, in the worst, base and best cases. This CBA was excluded from the systematic review due to the ex-ante economic assessment being conducted prior to program implementation.



Discussion and conclusion

The objectives of this report are modest—first, to assess current CBAs in crime prevention and criminal justice areas in Australia, second, to provide a systematic review of some of the more rigorous studies and third, to provide a tool to assess CBA.

Very few CBAs and only a few CEAs have been completed in the Australian criminal justice field. Although CEAs are valuable for determining the cost of averting a certain outcome and can therefore be used to compare competing projects, CEAs do not incorporate the monetary benefits of the outcome.

The majority of criminal justice CBAs and CEAs have been conducted in the United Kingdom and the United States. A number of the most significant ‘crime prevention’ studies have not been done by criminal justice or criminology field, but by the early childhood and/or health fields, where crime prevention is but one of a number of effects derived from early childhood or home visiting interventions. In one of the few reviews of CBAs conducted in Australia, the Australian Institute of Family Studies and the Melbourne Institute of Applied Economic and Social Research conducted a review of selected early childhood interventions (Wise et al. 2005). The report assessed program design and implementation,

evaluation design and evaluated program effectiveness by examining effect sizes. Among the studies assessed, there was significant variability in the quality of the design and methodology used. Small sample sizes, short follow-up periods and attrition rates plagued many of the other experimental designs. It was also clear that relatively short timeframes and an absence of sensitivity analyses are problems not isolated to criminal justice analysis.

Of the studies examined in this report, all had baseline data available, an intermediate follow-up period and had implemented reliable measures of program outcomes.

- For the studies assessed, follow-up periods ranged from six to 12 months for the Bowers et al. (2004) alley-gating intervention up to 15 years for the CPC program. Follow-up periods beyond 10 years are rare and are available for only a small number of programs, for example Perry Preschool and the Elmira Program. Of the nine programs assessed, only three studies had measured effects for longer than two years after the intervention period (Caldwell et al. 2006; Reynolds 2002; Roman et al. 2008). The remainder did not collect information on long-term follow-up.

- Even though only programs containing a control or comparison group were included in the systematic analysis, the comparability of the control/comparison group to the experimental group varied from program to program. The use of statistical matching provided greater assurance regarding the comparability between the two groups and all but one program included an appropriately matched comparison group and design methodology (Farrington et al. 2002). Farrington et al. (2002) noted, however, that groups differed significantly on a re-conviction index (the score used to match the experimental to control group).
- All but one of the studies were considered to have incorporated a representative sample of participants (Farrington et al. 2002). The majority of the studies had low attrition at follow-up (except for Robertson et al. 2001 —48% attrition; Acumen Alliance 2005— not reported), adequate statistical power (except for Daley et al. 2004; Robertson et al. 2001) and were considered to have included appropriate outcome measures (except for Caldwell et al. 2006). Further, with the exception of Acumen Alliance (2005), all studies reviewed were considered to have implemented an appropriate statistical analysis.
- When compared with the checklist for assessing economic evaluations, all studies included a well-defined question, were able to establish program effectiveness and contained generalisable results. The majority of studies also included a comprehensive description of alternatives, valued costs and outcomes credibly, and provided an incremental analysis of costs and consequences. Conversely, only one study (Reynolds et al. 2002) conducted a sensitivity analysis. Of the nine studies reviewed, six included all important and relevant costs and outcomes. Five of the nine studies were considered to have included study results incorporating all issues of concern to users. Four of the nine studies adjusted costs and outcomes for differential timing; the remaining five were unlikely to require adjustment due to the short timeframe for monetising costs and outcome after intervention was completed. Of the nine studies reviewed, all but three were considered to have measured costs and outcomes accurately.

A controversial point is the selection of the discount rate. This selected rate can vary substantially between assessments and can affect whether a program is assessed as cost-beneficial or not. The use of sensitivity analysis to compare cash flows at different discount rates is crucial for programs that have cash flows that extend many years after the treatment period. However, in general, only a limited number of CBAs incorporate a sensitivity analysis and in this systematic review only one included a sensitivity analysis (Reynolds et al. 2002).

Another concern is the success in scaling up small treatment programs to produce benefits on a large scale. More specifically, will treatment benefits be replicated on a larger scale? Many of the initial programs are conducted by researchers and trained professionals, therefore, the extension of the project on a larger scale may result in the programs being managed and conducted by less qualified individuals.

The majority of CBAs conducted on crime prevention and criminal justice programs relate to the economic assessment of drug courts, focusing primarily on the effectiveness of drug treatment on recidivism. In addition, the effect of situational prevention (alley-gating, street lighting, CCTV etc) in reducing crime has also been comprehensively examined. This is likely to be due to the ease in accessing a large group of individuals available to complete the programs in situational prevention programs and having ready access to prisoner records on conviction history, demographic factors and other identifying information obtained at intake for drug court participants.

Moving forward, a need exists for standardising measurement and valuation of benefits to enable comparison of cost-benefit ratios between programs. A standard list of core program benefits (criminal justice costs resulting from recidivism, avoided victimisation costs etc) for certain types of intervention would resolve many of the variations in what is included and excluded for each analysis. Itemising all individual benefits and the dollar value attributed to each benefit, and rationale behind these calculations, would allow greater comparison between programs and remove the issue of unsubstantiated final figures. Recognising

the impossibility of quantifying every program benefit, a standard list, with supplementary benefits in addition to core benefits, will allow programs to be compared on similar benefits included. Moreover, standardisation of benefits, based on rigorous and comprehensive estimates, would ensure a prevented assault in one CBA has the same avoided cost as an avoided assault in another CBA. Although this figure may be understated, as it likely to be a more conservative estimate, this would result in a more reliable average figure per crime type and result in better estimates of costs and benefits.

Comprehensive rigorous evaluation must be undertaken for the majority of programs if the criminal justice sector is to develop sustained investment in evidence-based crime prevention programs. This evaluation must be able to demonstrate not only that programs are effective but also that they provide greatest significant return on investment. To achieve the latter requires the inclusion of CBA as an integral part of program evaluation and further work to develop a more precise framework that can assist evaluators in estimating direct and indirect costs, as well as the tangible and intangible costs and benefits of crime prevention programs.

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Appendix A: Summary of nine case studies

Authors and date	Crimes targeted	Treatment setting	Sample size, type and attrition ^a	Length of intervention and follow-up ^b	Primary type of intervention	Evaluation design	Results ^c	Benefits measured (used in calculating cost-benefit ratio)	Cost-benefit ratio	Scientific methods score (1–5)	Evidence rating system	11 point Drummond score
Bowers, Johnson and Hirschfield (2004)	Burglary	Residential streets	Sample=362 homes, 3,178 gates installed	2–3 years 6–12 months	t=target hardening—installation of hardwearing lockable gates, c=no gates	Before–after, experimental, control	Burglary+	Crime victim expenses (direct) and CJS	0.96:1 and 1.86:1	3	9	8
Caldwell, Vitacco and Van Rybroek (2006)	Delinquent activity in general	Institution	Final sample=202 juveniles t=101, c=101 attrition=0%	Approx 12 months 4.5 yrs	t=cognitive and behavioural therapy, c=secured juvenile corrections institution	Before–after, experimental, control	Any offence+ Violent offence+ Felony offence+	CJS	7.18:1	3	9	6
Robertson, Grimes and Rogers (2001)	Delinquent activity in general	Community	Final sample=153 juveniles t1=61, t2=47, c=45 attrition=48% (original sample=293)	6 months 12 months	t1=intensive supervision and monitoring (ISM), t2=intensive outpatient counselling with cognitive-behavioural therapy (CBT), c=regular probation (RP),	Before–after, experimental, control	CJS+ (court referrals+ and days of detention+)	CJS	1.96:1	3	6	5
Roman, Chalfin, Reid and Reid (2008)	Recidivism	Community	Final sample=277 misdemeanour DUI offenders t1=136, c=141 attrition=0%	18 months 48 months	t1= therapeutic jurisprudence, c=regular jail time and fines	Before–after, experimental, control	Recidivism+	Crime victim expenses (direct) and CJS	3.44:1 after 24 month follow-up 1.25:1 after 30 months follow-up	3	10	9
Carey and Finigan (2004)	Recidivism	Community	Final sample=1,167 t=594, c=573 attrition=unknown (0%)	unknown 18–22 months	t=drug treatment for drug addicted offenders; c=traditional court processing	Before–after, experimental, control	Arrests+ Booking+ Court time+ Jail time+	CJS and victimisation costs	^d	3	9	10
Daley, Love, Shepard, Petersen, White and Hall (2004)	Recidivism	Institution	Final sample=831 inmates t1=41, t2=143, t3=30, t4=72, c=545 attrition=0%	t1=1 week t2=10 weeks t3=four months t4=6 months 24 months	t1=1 week session of drug/alcohol education, t2=30 outpatient group sessions, t3=4 sessions per week for 4 months, t4=full-time daily treatment for six months, c=no participation in tiered treatment programs	Before–after, experimental, control	Arrests+	Incarceration costs avoided	t1=CBA, t2=5.74:1, t3=3.18:1, t4=1.70:1	3	8	7
Reynolds, Temple Robertson and Mann (2002)	Delinquent and adult criminal activity	Community	Final sample=1,286 t=841, c=445 attrition=16 percent (original sample=1,539)	t1=1–2 years t2=1–3 years t3=4–6 years 15 years	Education and family support for children	Before–after, experimental, control	Education+ Academic achievement+ Crime and delinquency+ Child abuse+ Economic wellbeing+	School remedial services, CJS, child welfare costs, avoided victimisation costs Projected earnings and tax revenues	7.14:1, 6.11:1, 1.66:1	3	10	11
Farrington, Ditchfield, Gareth Hancock, Howard, Jolliffe, Livingston and Painter (2002)	Recidivism	Institution	Final sample=302 t=175, c=127 attrition=4% (original sample=314)	6 months 24 months	t=CBT skills training and military elements; c=standard youth offending institution	Before–after, experimental, control	Arrests+	CJS and victimisation costs	1.02:1, 5:1	3	7	9
Acumen Alliance (2005)	Recidivism	Community	Final sample=180 t=91, c=89 attrition=unknown	2 years 1 year	t=drug treatment for drug addicted offenders; c=traditional court processing (incarceration)	Before–after, experimental, control	Arrests+ Employment+	CJS, avoided victimisation, health costs, housing costs	5.81:1	3	7	7

a: c=control group; t=treatment group

b: The period of time in which program effects were evaluated after the intervention had ended

c: 0=no intervention effects; +=desirable intervention effects; -=undesirable intervention effects

d: The cost-benefit ratio was unable to be calculated as program investment required to process participants in the drug court group was less than investment in the 'business as usual' process, control group

n/a=not applicable